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for

Vol. 50

ACI *Proceedings*, 1954

(JOURNAL Vol. 25)

to provide a comradeship in finding the best ways to do concrete work of all kinds and in spreading that knowledge

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Every year the Institute receives many requests for the Title Page and Index of the annual *Proceedings* volume long after inquirers have received them; also many inquiries about binding annual sets of JOURNALS and buying bound volumes from the Institute. Within the covers of this Part 2 of the December JOURNAL are Title Page, Table of Contents, Concluding Discussion, and Indexes for the volume otherwise completed with the June 1954 JOURNAL. On the back cover is an announcement about the availability of bound volumes from the Institute and about "gathering" the contents of a year's JOURNALS, plus this Part 2, for binding a *Proceedings* volume. Please see announcement on the back cover.

PROCEEDINGS
AMERICAN CONCRETE INSTITUTE

Volume 50—1954

from JOURNAL OF THE AMERICAN CONCRETE INSTITUTE (Vol. 25)

September to December 1953, January to June 1954
(10 JOURNAL issues) and
Part 2 of December 1954

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ERRATA and ADDENDA

The following correction should be made in "Minimum Standard Requirements for Precast Concrete Floor Units (ACI 711-53)," ACI JOURNAL, September, 1953.

p. 15, line 16—¾ in. should read ⅜ in.

* * * *

The following correction should be made in "Concrete for Radiation Shielding," ACI JOURNAL, September, 1953.

p. 41, line 33—"An internal diameter. . . ." should read "An internal radius. . . ."

* * * *

The following correction should be made in "Correlation Between Laboratory Accelerated Freezing and Thawing and Weathering at Treat Island, Maine," ACI JOURNAL, October, 1953.

p. 162—the fourth line in part (a) under the heading "Differences Between Laboratory and Treat Island Exposures" should read "removed at about 45 months, . . . "

* * * *

The following correction should be made in the review "Effect of Entrained Air on Strength and Durability of Concrete Made with Various Maximum Sizes of Aggregates," ACI JOURNAL, "Current Reviews" section, October, 1953.

p. 184—the sixth and seventh lines in the last paragraph should read ". . . about 9 percent entrained air in the mortar fraction regardless of . . ."

* * * *

The following correction should be made in "Stirrup Design," ACI JOURNAL, "Letters from Readers" section, January, 1954.

p. 396—the first sentence in the third paragraph should read as follows:

"The writer's solution is exact and general, that is to say, the shear diagram may be a trapezoid as well as a triangle, whereas, the author's method is an approximation of the triangular case."

* * * *

In the pictorial series, "Notable Concrete Structures—Old and New," ACI JOURNAL, Feb. 1954, p. 476, the architect for the cafe built for the Builders' Exhibition, Hannover, Germany, should be Gutschow.

* * * *

In the pictorial series, "Notable Concrete Structures—Old and New," ACI JOURNAL, Feb. 1954, p. 498, the following information should be added to the material on the floating arch bridge over the Derwent River, Hobart, Tasmania. Although A. W. Knight conceived the idea of building this bridge, and actually took out a patent to cover the form of construction which was incorporated in it, the Department of Public Works, Tasmania, of which Mr. Knight was then chief engineer, commissioned David V. Isaacs as consulting engineer. In this capacity he designed the whole floating section of the bridge and the two abutments to which it was anchored, basing the design on data supplied by the Department of Public Works and Mr. Knight.

* * * *

The following correction should be made in "Fifty Years in the Technical Development of Concrete Pipe," ACI JOURNAL, Feb. 1954.

p. 518—The first and second sentences under the heading "Prestressed concrete pipe" should read as follows:

"In 1939 J. E. Miller and Lewiston Pipe Co. (now a part of Price Brothers Co.), Hillside, Ill., constructed several prestressed concrete pipe lines for the city of Hammond, Ind., to operate under a pressure of 40 psi. In 1941 this same company constructed a water line for the village of Melrose Park, Ill., and in 1943 constructed a prestressed concrete pipe line for the city of Chicago to withstand 100 psi. These pipe were made centrifugally by a process somewhat similar to Hume's and then wrapped with high tensile steel wire, which in turn was protected by a vibro-cast coating of cement-sand mortar 1 in. thick. The pipe for the city of Chicago were given a yard test of 200 psi in addition to a line test of 115 psi."

* * * *

In the pictorial series, "Notable Concrete Structures—Old and New," ACI JOURNAL, Feb. 1954, p. 524, the following information should be added to the material on the Walnut Lane Memorial Bridge, Philadelphia, Pa. G. Magnel and F. G. Riessauw were consultants to Preload Engineers, Inc. The Blaton-Magnel prestressing system was used.

* * * *

The following correction should be made in "Static and Dynamic Elastic Behavior of

Reinforced Concrete Beams," ACI JOURNAL, March, 1954.

p. 558—the sixth equation down from the top of the page, immediately preceding Eq. (17), should read:

$$\sum_{n=1}^{n=\infty}\left[\omega_n^2\, z_n(t) + \frac{dz^2}{dt^2}{}_n(t) - w_n(t)\right]$$

$$\int u_n(x)\, u_{n1}(x)\frac{c}{g}dx = 0$$

* * * *

The following correction should be made in "Basic Design Criteria for Concrete Gravity and Arch Dams," ACI JOURNAL, April, 1954.

p. 667—the third sentence under "Combinations of loadings—Criteria" should read as follows:

"..... load combination (D below) and the smaller safety factor prescribed used."

* * * *

The following corrections should be made in "Cellular Concretes," ACI JOURNAL, May and June 1954.

p. 775 (May JOURNAL)—The second line under the heading "Commercial production and applications" should read as follows: ". . . where virtually all lightweight concrete masonry. . . "

p. 787 (May JOURNAL)—The section heading "Mixing of cellular concrete using the excess water method," now italicized, should be in boldface type, because it is not, strictly speaking, a type of "foamed" cellular concrete, but is a type itself, fitting neither the gas nor the foam designations.

p. 831, Table 7 (June JOURNAL)—A shrinkage value (column 9) is given as "1.14" in the bottom line of the fourth group of data in the table. The correct value is "0.14" percent.

* * * *

The following correction should be made in "Selection and Design of Prestressed Concrete Beam Sections," ACI JOURNAL, November, 1953.

p. 223—the figure above the caption "Fig. 7—Stress distribution for the chosen section in example" should be the following

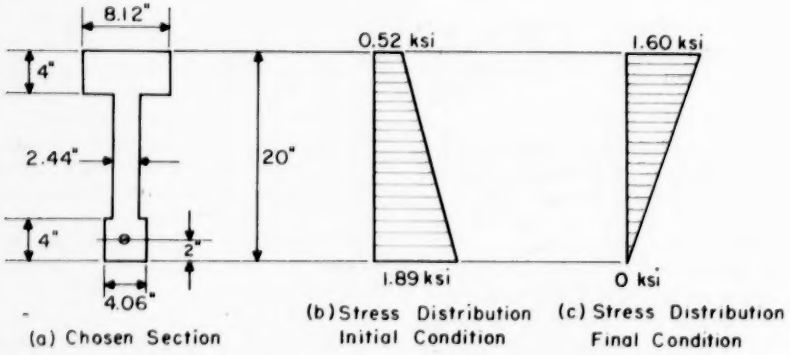

Fig. 7—Stress distibution for the chosen section in example

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Current ACI Standards

(As of December, 1954)

Listed below are ACI standards in effect at the time the closing portions of *Proceedings* V. 50 are published in the ACI JOURNAL, Part 2 December 1954:

Manual of Standard Practice for Detailing Reinforced Concrete Highway Structures (ACI 315A-53)

A publication of large format, bound to lie flat. 41 pages: $2.75 per copy ($1.50 to ACI Members)

Manual of Standard Practice for Detailing Reinforced Concrete Structures (ACI 315-51)

A publication of large format, bound to lie flat. 55 pages: $3.00 per copy ($1.75 to ACI Members)

Building Code Requirements for Reinforced Concrete (ACI 318-51)

64 pages in covers: 75 cents per copy (40 cents to ACI Members)

Standard Specification for the Design and Construction of Reinforced Concrete Chimneys (ACI 505-54)

48 pages in covers: $1.00 per copy (50 cents to ACI Members)

Specifications for Concrete Pavements and Bases (ACI 617-51)

24 pages in covers: 50 cents per copy (40 cents to ACI Members)

Recommended Practice for the Application of Mortar by Pneumatic Pressure (ACI 805-51)

12 pages in covers: 50 cents per copy (40 cents to ACI Members)

Recommended Practice for the Application of Portland Cement Paint to Concrete Surfaces (ACI 616-49)

16 pages in covers: 50 cents per copy (40 cents to ACI Members)

Recommended Practice for Winter Concreting Methods (ACI 604-48)

20 pages in covers: 50 cents per copy (40 cents to ACI Members)

Minimum Standard Requirements for Precast Concrete Floor Units (ACI 711-53)

16 pages in covers: 50 cents per copy (40 cents to ACI Members)

Recommended Practice for the Construction of Concrete Farm Silos (ACI 714-46)

16 pages in covers: 50 cents per copy (40 cents to ACI Members)

Recommended Practice for Selecting Proportions for Concrete (ACI 613-54)

16 pages in covers: 50 cents per copy (40 cents to ACI Members)

Specifications for Cast Stone (ACI 704-44)

4 pages: 35 cents per copy

Recommended Practice for the Use of Metal Supports for Reinforcement (ACI 319-42)

4 pages: 35 cents per copy

Recommended Practice for Measuring, Mixing, and Placing Concrete (ACI 614-42)

30 pages in covers: 50 cents per copy (40 cents to ACI Members)

Discussion of a report by ACI Committee 613:

Proposed Recommended Practice for Selecting Proportions for Concrete*

By C. W. BRITZIUS, E. L. HOWARD, G. W. GREENE, G. E. LEAVITT, W. E. MOULTON, HARRY E. THOMAS, and COMMITTEE

By C. W. BRITZIUS†

I have studied the committee's proposed recommended practice which is to replace "Recommended Practice for the Design of Concrete Mixes (ACI 613-44)." I feel that ACI 613-44 was excellent and that the committee should have directed its efforts to improving and refining it and not attempted to bring something forth that is new and less usable. It is certainly true that the b/b_o is an old concept and is the major factor in the selection of proportions for a concrete mix. Specifically, I do not care for the specification because I feel that it is less accurate as to the unit water content.

I would like to call attention to how and by whom this standard will be used. Concrete for large government jobs is laboratory designed by the trial mix method, but a large proportion of the concrete placed in the United States is on small jobs. Certain information in the standard will be used by the architect or engineer who writes the specification, certain other information is for the laboratory technician whose attention is called to the factors affecting the mix proportions. The primary use of the standard will be by contractors, ready-mixed concrete plant operators, and job inspectors who wish to calculate the mix for their particular job. On these jobs, no facilities usually exist for making mix adjustments. My experience has been that a man who proportions the mix in the laboratory, and who appreciates job conditions, can proportion a mix that will require little adjusting in the field except possibly a small change in the percent of fine aggregate. I do not care for the section giving proportions for small jobs, for it adds confusion to the specification and impairs its usefulness. If something of this nature must be added, I would suggest including a table of mixes of varying cement contents in which the water-cement ratio and the total aggregate per sack be given, but to be proportioned in the field as to the ratio of fine to coarse.

I like the ACI 613-44 standard and would like to see it improved upon by improving the table of unit water requirements and by defining more accurately what is meant by angular and round particles. An accurate evaluation

*ACI Journal, Oct. 1953, *Proc.* V. 50, p. 105. Disc. 50-6 is a part of copyrighted Journal of the American Concrete Institute, V. 26, No. 4, Dec. 1954, Part 2, *Proceedings* V. 50.

†Twin City Testing and Engineering Laboratory, St. Paul, Minn.

of the voids in the coarse aggregate can be made in the laboratory on the basis of void content. I would recommend inclusion of the following type of table to be used with Table 5:

Size of coarse aggregate	Voids in rounded coarse aggregate, percent	Voids in angular coarse aggregate, percent
¾ in.–No. 4	36	46
1 in.–No. 4	34	44
1½ in.–No. 4	32	42
2 in.–No. 4	30	40

Mix proportions by calculation are fairly accurate if based on a minimum cement content specification, but may be considerably in error if the specification is open as to cement content.

By E. L. HOWARD, G. W. GREENE, G. E. LEAVITT, and W. E. MOULTON*

For some years we have used the ACI procedure for mix proportioning with sand and gravel percentages determined from fineness modulus tables published by the National Ready Mixed Concrete Assn. Concrete resulting from this method of proportioning is good and satisfies all concerned.

The proposed method of proportioning has been put to immediate service in our organization to test its effectiveness with our materials. We have set up mixes using our old and the proposed methods of proportioning. Materials used are typical of the aggregates supplied concrete batching plants in three areas of California (Table A).

The first departure from our normal procedure was found in Table 3. We have used the water contents that will yield 4- to 5-in. slumps, varying them from Table 3 as experience dictates. We have always known that air is entrapped in normal concrete. Our estimates would agree with Table 3. However, we have not taken this entrapped air into account because the 0–2 percent volume increase is not a job problem.

The mixes shown in Tables B, C, and D have been proportioned with no regard for the air suggested in Table 3, Table E compares some mixes with the entrapped air included in the design.

TABLE A—MECHANICAL ANALYSIS OF AGGREGATES

Source	Eliot	Lemon Cove	Fair Oaks
Sand			
Fineness modulus	3.00	3.00	3.00
Specific gravity	2.70	2.68	2.70
Gravel			
Fineness modulus	6.68	6.96	7.00
Specific gravity	2.70	2.70	2.80
Rodded weight per cu ft, lb	106	104	113

Using Table 6 is difficult for us because the FM's shown are for sand much finer than we normally use. Our sands quite often have an FM of 3.4. To be of use to us at least volumes for 3.2 FM should be added.

The batch man will like the new mixes because the coarse aggregate

*Pacific Coast Aggregates, Inc., San Francisco, Calif.

TABLE B—CONCRETE MIXES PROPORTIONED USING ELIOT AGGREGATES

Water, gal.	Cement, lb	Dry sand, lb	Dry gravel, lb	Maximum aggregate, in.	Method used
42	658	1345	1690	¾	Proposed
42	658	1070	1980	¾	Old
42	564	1435	1690	¾	Proposed
42	564	1250	1870	¾	Old
42	495	1485	1690	¾	Proposed
42	495	1400	1780	¾	Old
42	440	1530	1690	¾	Proposed
42	440	1485	1740	¾	Old

TABLE C—CONCRETE MIXES PROPORTIONED USING LEMON COVE AGGREGATES

Water, gal.	Cement, lb	Dry sand, lb	Dry gravel, lb	Maximum aggregate, in.	Method used
42	658	1250	1780	1	Proposed
42	658	1080	1955	1	Old
42	564	1330	1780	1	Proposed
42	564	1215	1905	1	Old
42	495	1390	1780	1	Proposed
42	495	1310	1865	1	Old
42	440	1440	1780	1	Proposed
42	440	1385	1835	1	Old

TABLE D—CONCRETE MIXES PROPORTIONED USING FAIR OAKS AGGREGATES

Water, gal.	Cement, lb	Dry sand, lb	Dry gravel, lb	Maximum aggregate, in.	Method used
38	595	1305	1955	1	Proposed
38	595	1370	1880	1	Old
38	510	1375	1955	1	Proposed
38	510	1470	1860	1	Old
38	447	1430	1955	1	Proposed
38	447	1525	1855	1	Old
38	397	1470	1955	1	Proposed
38	397	1575	1845	1	Old

TABLE E—CONCRETE MIXES PROPORTIONED USING ELIOT AGGREGATES

Water, gal.	Cement, lb	Dry sand, lb	Dry gravel, lb	Maximum aggregate, in.	Method used	Cost per cu yd, dollars
42	658	1255	1690	¾	Proposed—Air	8.38
42	658	1345	1690	¾	Proposed—No air	8.46
42	658	1070	1980	¾	Old	8.46
42	440	1440	1690	¾	Proposed—Air	6.72
42	440	1530	1690	¾	Proposed—No air	6.80
42	440	1485	1740	¾	Old	6.81

weight for a given maximum size will always be the same.

The proposed method brings into use a test we have long since discarded. This is a day of separated gravel sizes in the batch plant. The exact proportions of each size must be determined before the unit weight is taken because Table 6 is based on combined coarse aggregates. We now determine these proportions and the sand proportions from the same tables. The unit weights are not at all useful in this procedure.

The Eliot gravel and sand used for proportioning in Table B are average for the material supplied the San Francisco Bay area. We believe the proposed method yields a little too much sand in the richer mixes.

Lemon Cove aggregate is used in concrete represented by Table C. Here

the coarse aggregate is angular and the slight increase in sand is not objectionable. Fair Oaks aggregate from the Sacramento area is used in mixes shown in Table D. The leaner mixes are undersanded using the proposed method.

The mixes shown in Tables B, C, and D do not account for entrapped air. If the volume of air had been considered, even less sand would have resulted. The leaner mixes would, in each area, have been undersanded.

The cost of concrete is unaffected by the new proportions unless the entrapped air is figured in the design. The addition of air means 90 lb less sand and 8 cents less cost for materials when using ¾-in. maximum aggregate (Table E). This is a considerable saving in large jobs. If entrapped air is taken into account and at the same time the customer is assured a full cubic yard of concrete, testing for air content most certainly must be done. Many cubic yards of concrete would have to be placed to earn the cost of such testing.

We like the recommended practice for selecting proportions for concrete for its (1) simplicity of calculation, and (2) uniform batch weights in case of coarse aggregates.

We dislike the recommended practice for its (1) inadequacy of Table 6 in the case of coarse sand, and (2) use of the long-since abandoned unit weight test.

By HARRY E. THOMAS*

The need for a simplified method of proportioning concrete mixes was a long felt need before "Recommended Practice for the Design of Concrete Mixes (ACI 613-44)" made its appearance. Adopted by ACI, it was enthusiastically accepted by this laboratory because of its simplicity and its exactness. We had employed Abrams' method since 1923 and still stoutly defend it as being accurate, but it is bulky to use and is awkward to the novice. It lent itself to error in all its steps from real mix to nominal mix to field mix, with shrinkages and bulking factors and finally to proportions by weight and, in later years, checking the whole thing by absolute volumes.

When ACI 613-44 came along it was so utterly simple that it immediately caught our eye and we checked the design of several mixes against Abrams'. The sand content invariably checked within usually 2 percent and the cement factors were always close so the new method was adopted. It's no trick at all to turn a green man loose and get a quick, accurate design.

Now comes a proposed recommended practice to confuse us, for we thought ACI 613-44 was the ultimate. I think we might as well have adopted the Goldbeck method outright. Both are primerish, and as such, have their place among students; but for the busy engineer who has to design concrete mixes, ACI 613-44 is more to the point and easier to use. Table 5 in that standard leads the design man right to the point, unswervingly, as the dry-rodded weight method does not, for I know how variable the dry-rodded weight can be with two different men.

*Pacific Gas and Electric Co., Emeryville, Calif.

COMMITTEE'S CLOSURE

Messrs. Howard, Greene, Leavitt, and Moulton have raised some questions regarding entrapped air, fineness modulus of sand, and rodded unit weight of combined coarse aggregate as treated in the proposed standard.

Entrapped air cannot be ignored in a study of the effect of entrained air on the properties of concrete. Intentionally entrained air is in excess of the amount normally entrapped in the mix. Only the entrained air, in proper distribution, improves workability and increases the resistance of concrete to freezing and thawing. The recommended average total air contents listed in Table 3 of the proposed standard corresponds to about 7 percent intentionally entrained air in the mortar of the concrete in addition to air normally entrapped by the concrete.

The committee did not think it advisable to show in Table 6 FM's higher than those listed in ASTM specifications for fine aggregate. The values listed in Table 6 of the proposed standard may be extrapolated for sands having an FM as high as 3.4 as used by Mr. Howard.

We agree that if the coarse aggregate is separated into its component sizes, as it usually is, the relative amount of each size fraction to be used in the mix must be determined before the rodded unit weight of the combined coarse aggregate can be ascertained. Usually it is desirable, when suitable workability is obtained and specifications are not violated, to use the materials in the proportions produced from the pit or quarry. Using this grading as a start, successive adjustments may be made to obtain a suitably workable and economic mix.

The sand percentages listed in Table 5 of ACI 613-44 were considered in establishing values listed in Table 6 of the proposed standard. Any set of values such as these can be given for average conditions only. Also, inasmuch as cement usually contributes more to workability than does an equal volume of sand, the values from Table 6 yield some rich mixes that appear oversanded and lean mixes that appear undersanded, as is reported by Mr. Howard. The values listed in Table 6 are intended for use in developing the first trial; usually adjustments must be made to obtain a mix that is completely satisfactory for the materials used.

In answering Mr. Thomas' criticism, it should be pointed out that with the exception of the portions on air-entrained mixes, the proposed standard differs from the 1944 standard in only one important point, namely, the method for selecting the proportions of fine and coarse aggregate. In the 1944 standard the minimum amount of sand required to produce workability, the amount of water to give the necessary fluidity, and the water-cement ratio required for strength and/or durability are selected from tables given in the report. From these values the amount of coarse aggregate in the mix is computed. In the proposed standard the percentage of dry-rodded coarse aggregate which can be used in a given volume of concrete is selected from a table. For example, if the dry-rodded weight of the coarse aggregate graded up to 1½-in. maximum is 100 lb per cu ft, the table shows that 74

percent of this or 74 lb can be used per cu ft of concrete with a sand having an FM of 2.60. The volume remaining in the cubic foot is filled by sand, cement, water, and air. The water and cement are determined as in the 1944 standard and the amount of sand, if no entrained air is involved, fills the rest of the space. This method permits, within usual variations, a ready interchange of cement, water, sand, and entrained air without materially affecting the workability. Adjustments where entrained air is involved are facilitated.

In the proposed method of proportioning, the effect of particle shape and grading of the coarse aggregate is accommodated by reference to the rodded unit weight. For a given grading the unit weight of an angular aggregate of a given specific gravity is usually less than that of a rounded aggregate having the same specific gravity because the rounded aggregate fits together more tightly with a smaller percentage of voids among the particles. Because of this, less angular coarse aggregate is used in a mix by this method than rounded coarse aggregate. Likewise, the unit weight of poorly graded aggregate is less than that of well graded aggregate from the same plant, and less poorly graded material by the rodded unit weight reference is used in the concrete mix.

Rodded unit weight varies slightly when obtained by different operators, but this variation causes errors no greater than are obtained through the use of the average sand percentages listed in Table 5 of ACI 613-44. In ACI *Proceedings* V. 40 W. H. Price discusses the errors which might result from use of Table 5 of ACI 613-44 and rodded unit weights as recommended in the proposed standard.*

Table 5 of ACI 613-44 lists water contents for rounded and for angular aggregate. The proposed standard lists average values for these because aggregates and cements from various parts of the country require widely different water contents and there may be a greater difference between river-worn aggregates than between a river-worn aggregate and a crushed aggregate. At Medicine Creek Dam in Nebraska, only 25 gal. of water per cu yd of concrete were required to produce a 3-in. slump for concrete containing aggregate graded up to 1½-in. maximum size, whereas on the Friant-Kern Canal, in California, 37 gal. per cu yd were required for producing the same slump for aggregates of the same maximum size and produced under the same aggregate specifications. The aggregates at both projects were river-worn sand and gravel, which, to the naked eye, appeared to be similar in particle shape. The proposed method is intended to select a starting mix. Only average materials and conditions would produce a mix requiring no adjustment in the water content.

Because of the range in void content that might be obtained even with aggregate of one class and because of the small amount of time and effort required to make the rodded unit weight determination, the committee

*Discussion of "Proposed Recommended Practice for the Design of Concrete Mixes," ACI JOURNAL, June 1944, *Proc.* V. 40, p. 116-10.

decided against the inclusion of a table such as that suggested by Mr. Britzius.

The section for the small job has not been changed from that given in ACI 613-44 except for addition of values for concrete containing entrained air.

Discussion of a paper by Herbert E. Worley and Richard C. Meyer:

Development of a Cell for the Installation of Electrical Resistance Strain Gages in Concrete*

By LYLE K. MOULTON and ARON L. MIRSKY and AUTHORS

By LYLE K. MOULTON and ARON L. MIRSKY†

The authors have presented an interesting method of waterproofing and protecting SR-4 strain gages so that they can be used for the measurement of strains in concrete pavements. Although SR-4 strain gages have obviated many of the difficulties involved in the determination of strains, there are three problems which must be overcome before the gages can be used in a material such as concrete. The gages must be waterproofed and protected from mechanical damage, and they must be compensated for changes in length due to temperature. The methods described by the authors seem to provide solutions for the first two problems, but there is some question as to whether or not the method of temperature compensation used is entirely adequate.

On the basis that the strains in the top and bottom of the slab seem to be equal and opposite in sign, it was possible to pair together the gages in separate arms of the bridge. This doubles the bridge output and is an excellent method of providing for the measurement of small strains. However, with the gages arranged in this manner and installed in a concrete slab, adequate temperature compensation is not easy to achieve.

The purpose of dummy or temperature compensating gages is to eliminate automatically (via the appropriate circuitry) any strain effects in the active gages due to temperature changes; to accomplish this, they should be identical with the active gages in everything except exposure to strains due to the loading under investigation. This implies that they should not only be attached to the same type of material as the active gages but that the material to which they are attached should be subject to the same temperature deformations and restraints as is the material under test. It does not seem that this has been done in the tests under discussion.

In the first place the dummy gages were attached to bars of invar which does not have the same thermal coefficient of expansion as concrete. In the second place the thermal effects in the dummy gages were not subject to the

*ACI JOURNAL, Oct. 1953, *Proc.* V. 50, p. 121. Disc. 50-7 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 26, No. 4, Dec. 1954, Part 2, *Proceedings* V. 50.

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same restraint which affects the slab movement and manifests itself as subgrade friction. There is no doubt that these factors are made less objectionable by the fact that they tend to compensate for each other. It is assumed that this was the reason that invar was chosen for use in the dummy elements. For short term tests with small variations in temperature it is likely that no significant temperature effect would be noticeable. However, should this method be applied using continuous recording equipment for some period of time, the uncertainty involved in the factors of large temperature change and unknown subgrade friction would leave the results open to question.

A method which would eliminate much of the uncertainty would be to cast an identical slab adjacent to, but separated from, the slab under test. The compensating gages could be arranged in this slab in the same manner as the active gages except that this slab would be unloaded. A possible objection to this method could be that there is some uncertainty as to whether or not the two slabs would be at the same temperature. In addition, the expense would be fairly high.* The writers feel that the additional expense would be justified if the type of gage installation described is used to make a more detailed long term investigation, something that has long been needed.

AUTHORS' CLOSURE

Messrs. Moulton and Mirsky were concerned with the adequacy of compensation afforded by the dummy gages.

The problem of adequate compensation for electrical resistance strain gages installed in concrete is a difficult one. To truly compensate, the dummy gages must be subjected to the same conditions as the active gages except that they should not be strained due to the phenomena being measured.

No satisfactory method of achieving full compensation was available at the time of the installation. The dummy gages on invar metal were installed however to provide a reference from which to measure changes in length of the concrete which occur over longer periods of time. Invar metal has a very low coefficient of expansion and virtually remains at a constant length over quite a large temperature range. Because these dummy gages are at the same temperature as the active gages, the changes in gage wire resistance due to temperature is effectively balanced out.

A change in strain, using the dummy gages as a reference, is therefore a measure of the shortening or elongation of the concrete in the gage area. These apparent strains cannot be converted into stresses because they are not necessarily due to applied loads. The changes in length of the concrete from top to bottom of the slab might prove useful in the investigations of warping and the expansion and contraction of concrete slabs with temperature or changing moisture conditions.

*This could, of course, be eliminated by using one lane of a two-lane highway as the slab with the dummy gage, provided the two lanes were separated by a longitudinal expansion joint and traffic was allowed only in the active lane.

For strains which occur over short periods of time, such as individual axle loadings, no temperature compensation is necessary. If a continuous recording of strains due to axle loadings were desired, some more elaborate method of compensation would be required.

After being installed more than two years the gages are still maintaining a high resistance between the gage and gage cells. Limited tests which have been made recently show that all gages are still responding to strains which occur with axle loadings.

Discussion of a paper by Thomas B. Kennedy and Katharine Mather:

Correlation Between Laboratory Accelerated Freezing and Thawing and Weathering at Treat Island, Maine*

By STANTON WALKER and D. L. BLOEM, T. F. WILLIS and L. T. MURRAY, and AUTHORS

By STANTON WALKER and D. L. BLOEM†

The paper "Correlation Between Laboratory Accelerated Freezing and Thawing and Weathering at Treat Island, Maine" presents again‡ the concept of "thermal compatibility" as influencing the durability of concrete. That concept is that the greater the difference between the thermal coefficients of the coarse aggregate and mortar, the less able is the concrete to resist temperature changes and particularly freezing and thawing.

We questioned the validity of that concept in our discussion of Callan's paper‡ and we presented data from carefully planned and controlled tests in the paper by Walker, Bloem, and Mullen in the April 1952 ACI JOURNAL§ which substantiated our position. The data in the present paper by Kennedy and Mather, although presented in a form to support the concept, if taken at their face value, provide spectacular evidence of its lack of validity.

DFE VERSUS ΔC

Fig. 11, 13, and 15 present the individual test results plotted in a diagram for which the abscissas are differences in thermal coefficients between mortar and coarse aggregate (ΔC) and the ordinates are "durability factors" (*DFE*), a measure of the resistance of the concrete to freezing and thawing based on measurements of dynamic moduli of elasticity. The data in these diagrams show correlation coefficients ranging from -0.312 to $+0.333$ which are recognized by the authors as indicating no significant relationship. Regression lines for two of the diagrams indicate a trend of increased *DFE* with increased ΔC—a relationship opposite to that on which the thermal compatibility concept is based, but indicated by the correlation coefficient not to be signifi-

*ACI JOURNAL, Oct. 1953, *Proc.* V. 50, p. 141. Disc. 50-9 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 26, No. 4, Dec. 1954, Part 2, *Proceedings* V. 50.

†Director of Engineering and Assistant Director of Engineering, respectively, National Sand and Gravel Assn. and National Ready Mixed Concrete Assn., Washington, D. C.

‡See: Weiner, Albert, "A Study of the Influence of Thermal Properties on the Durability of Concrete," ACI JOURNAL, May 1947, *Proc.* V. 43, p. 997; Callan, E. J., "Thermal Expansion of Aggregates and Concrete Durability," ACI JOURNAL, Feb. 1952, *Proc.* V. 48, p. 485; "The Relation of Thermal Expansion of Aggregates and the Durability of Concrete," *Bulletin* No. 34, Waterways Experiment Station, Corps of Engineers, U. S. Army, Vicksburg, Miss.; and "Laboratory Investigation of Certain Limestone Aggregates for Concrete," *Technical Memorandum* No. 6-371, Waterways Experiment Station, Corps of Engineers, U. S. Army, Vicksburg, Miss.

§Walker, Stanton, Bloem, D. L., and Mullen, W. G., "Effects of Temperature Changes on Concrete as Influenced by Aggregates," ACI JOURNAL, Apr. 1952, *Proc.* V. 48, p. 661.

cant. The third showed a trend toward decreased *DFE* with increased ΔC, but also with an unsignificant correlation coefficient. Data from these same test results after "adjustment" are plotted in Fig. 12, 14, and 16 in such a manner as to show highly significant correlation coefficients and to support the thermal compatibility concept. The method for adjusting the data was first used, so far as we know, by E. J. Callan in the closure to discussion of his paper, previously cited. It is completely erroneous as will be shown in this discussion.

The method of adjustment used by Callan is referred to on p. 504-16 of the closure to discussion of his paper as "a somewhat intricate weighting of the observed *DFE*'s, the procedure for which is rather too detailed for description here." It is described briefly by Kennedy and Mather on p. 158 of the paper under discussion. We will not burden these proceedings by repeating a description of it here. We urge the reader to study the method carefully. Its application requires that the nature of the relationship to be "discovered" *must be assumed in advance.* That is to say, if it is assumed that *DFE* should *decrease* as ΔC increases, then the method of adjustment causes the data to assume that form. On the other hand, if one chooses to assume that *DFE* should *increase* as ΔC increases, then the application of the method of adjustment will cause the data to assume *that form.*

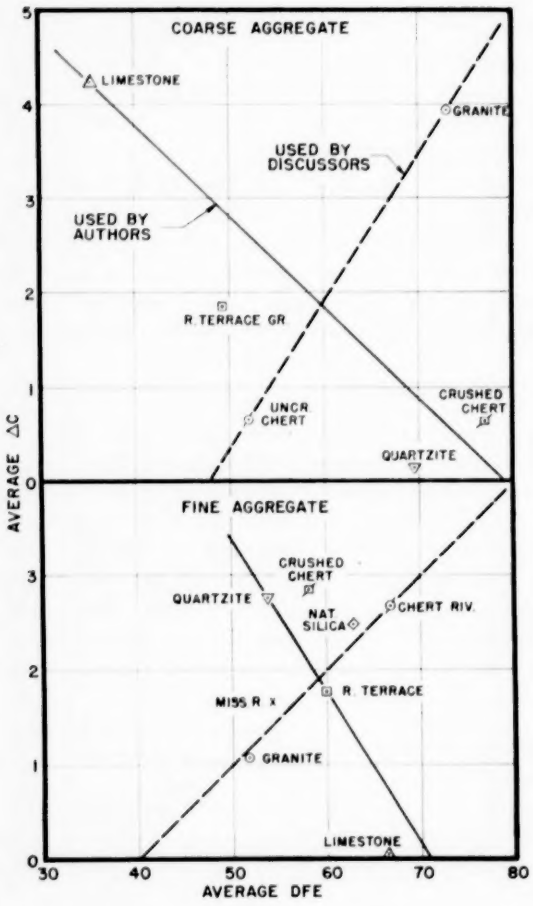

Fig. A—"Adjustment" curves

This is well illustrated in Fig. A and B. Fig. A is the working diagram for making the adjustment; the top chart is for adjusting values to "correct" for coarse aggregate and the bottom chart is for adjusting values to "correct"

for fine aggregate. The solid line represents the *prior* assumption that *DFE decreases* as ΔC increases and is drawn as Callan would draw it. The broken line is based on the *prior* assumption that *DFE increases* as ΔC increases.

With the adjustments made precisely as done by Callan and later described by Kennedy and Mather, using first the solid line and then the broken line, the diagrams shown in Fig. B result. The regression line at the top shows *DFE* to decrease as ΔC increases, with a highly significant correlation coefficient of 0.903 (the authors' calculations showed 0.847). The lower regression line shows an *exactly opposite* relationship with a substantially equally significant correlation coefficient of 0.857. It is clear, therefore, that the authors' Fig. 12, 14, and 16 are completely invalid and prove nothing with respect to the "thermal compatibility" concept.

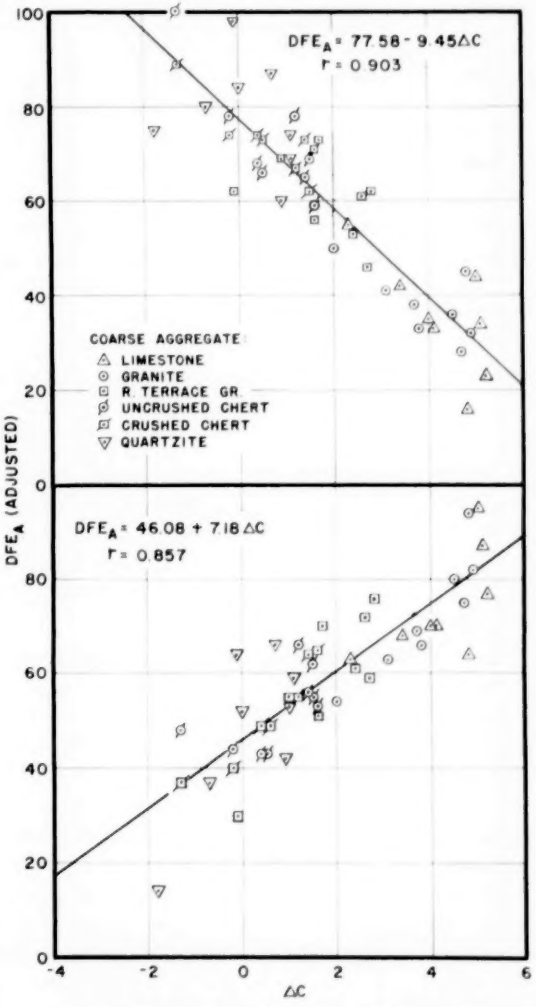

Fig. B—Relationship of "adjusted" *DFE* to ΔC

LABORATORY VERSUS TREAT ISLAND EXPOSURES

The objective of the Kennedy-Mather paper was to compare the effects on concrete of laboratory freezing and thawing and storage at Treat Island. Both of these exposures have been described adequately elsewhere.* Single batches of concrete, with two exceptions where there were two batches, were made for each of 48 different combinations of aggregates. Companion specimens from each batch were subjected to freezing and thawing in the laboratory and stored at Treat Island.

*Wuerpel, C. E., and Cook, H. K., "Automatic Accelerated Freezing-and-Thawing Apparatus for Concrete," *Proceedings*, ASTM, V. 45, 1945, p. 813; and Cook, H. K., "Experimental Exposure of Concrete to Natural Weathering in Marine Locations," *Proceedings*, ASTM, V. 52, 1952, p. 1169.

The authors conclude that the data show "no over-all correlation between the laboratory and ..... field beams," and then proceed with explanations of why the different concretes acted as they did. However, their acceptance of the relationship based on the adjusted data shown in Fig. 12, 14, and 16 (with highly significant correlation coefficients) denies their own conclusion. A diagram of *DFE*'s from laboratory freezing and thawing tests versus Treat Island storage, based on their adjusted regression lines, would furnish an excellent relationship showing the destructiveness of the laboratory freezing and thawing test to be closely related to that of storage at Treat Island. Here again, however, the relationship can be made to conform to a diametrically opposed notion, as will be demonstrated below.

At the risk of belaboring Callan's system of adjustment, we make one more application of it. In Fig. C, *DFE*'s from laboratory freezing and thawing have been plotted against "adjusted" *DFE*'s from the Treat Island exposure. The unadjusted data are shown in the authors' Fig. 6 with an unsignificant correlation coefficient of 0.050. We have no criticism of that diagram.

In Fig. C we have adjusted the data in accordance with the system developed by Callan—first, making the *prior* assumption that the two sets of *DFE*'s should be *directly* proportional to each other and, second, that they should be *inversely* proportional. Observe that the two diametrically opposed relationships are made to result, each with a highly significant correlation coefficient (0.784 and 0.819). We present these relationships only to illustrate the complete lack of validity of such a system of adjustment.

GENERAL COMMENT

If the preceding criticism were based on nothing more fundamental than irritation with erroneous conclusions, it would not be justified. But, the thermal compatibility concept has been used to reject aggregates unjustifiably and it could be used again! In their hypotheses, to be discussed later, the authors use the concept to explain why some of the concrete specimens acted as they did.

We feel that the ghost of thermal compatibility must be laid until there is tangible evidence of its existence. Let us emphasize that our discussion is based entirely on the data in the Kennedy-Mather paper; we will study, without prejudice, any further data which may be presented in the future on this important subject of thermal properties.

Up to this point, our comments have been directed almost entirely to the errors that can result from adjusting data. Data may be studied, arranged, analyzed, and interpreted, but they should never be adjusted—and especially with any preconceived idea of what they should show. We now come to a more fundamental criticism of the investigation. It is not conceivable to us that these data could have been expected to show any relationship between durability and any characteristic of the aggregate. First of all, with two exceptions, there was only one batch for each combination. For those two

exceptions, the duplicate tests showed such poor reproducibility as to indicate that attempts to compare the performance of individual aggregate combinations would be futile. Further, we are unable to understand how concretes ranging in cement factor from 4.56 to 6.66 sacks per cu yd (according

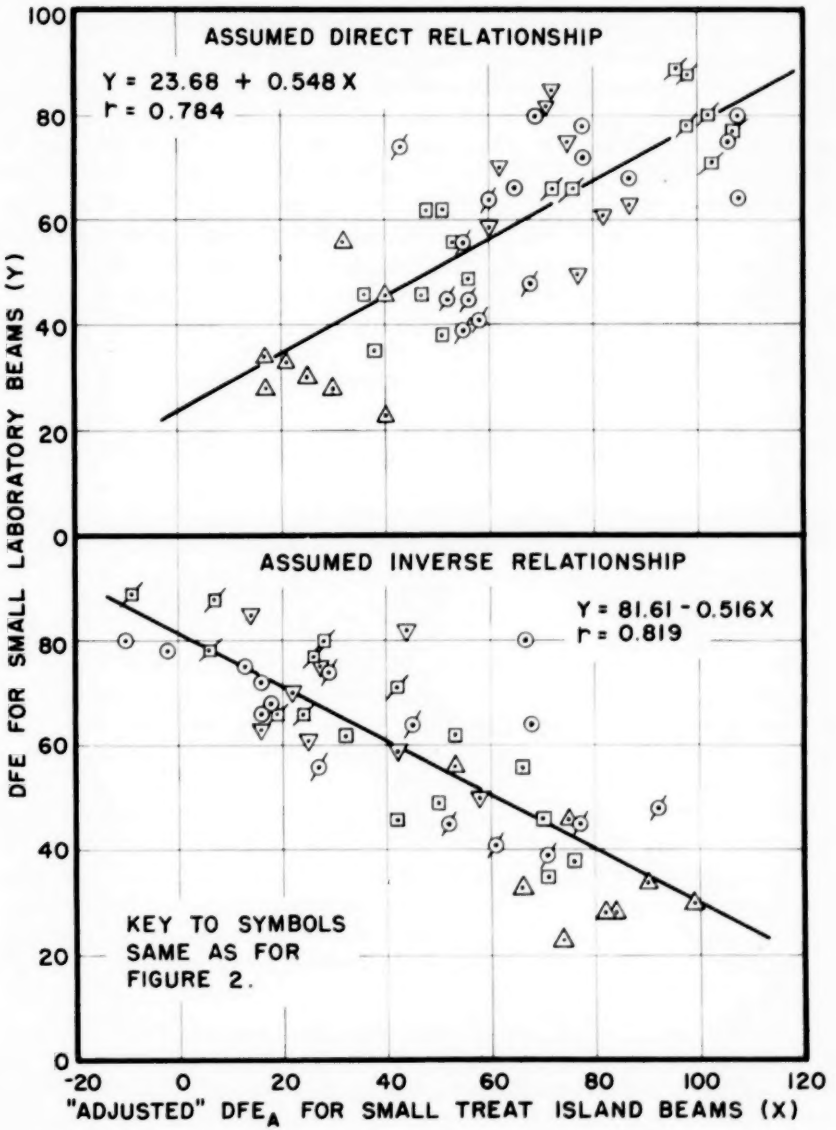

Fig. C—Relationships between *DFE* of laboratory beams and "adjusted" *DFE* of Treat Island beams

to Table 2), and in water content from 25 to 37 gal. per cu yd, can be considered to be comparable.

The only common features of these concretes were: (1) the same water-cement ratio; (2) identical gradings of coarse aggregates; and (3) similar gradings of fine aggregates. On the face of it, that might suggest that they have a great deal in common. But is that so? Granted that the water-cement ratio is the most important factor in fixing the quality of the cement paste. But are two concretes, one of which has approximately 5.5 cu ft of paste per cu yd and the other 8.1 cu ft, comparable? Are two concretes comparable if one of them contains 37 gal. of water per cu yd and the other 25 gal., the water occupying 18 percent of the volume of the concrete in one case and 12 percent in the other?

Recognizing the impossibility, in a precise sense, of making two concretes from different aggregates completely comparable, we believe that a much closer approach to practical comparability could have been made by proportioning the different aggregate combinations to have the same cement factor.

THE AUTHORS' HYPOTHESES

The "hypotheses to explain behavior" of aggregates appear to be somewhat less than convincing, especially when the lack of comparability of the different concretes in the single-batch results is considered. The authors say that the limestone fine aggregate gave poor results at Treat Island because it swelled. But, on the other hand, they offer as explanation of the good performance of limestone coarse aggregate also, the fact that it swelled. Further, it appears that sometimes alkali reactivity promoted durability and sometimes it did not.

Other similar explanations could be cited and we were tempted to examine them in detail. The futility of doing so impressed itself on us when we considered the lack of comparability of the concretes. Nevertheless, there may be some justification for speculating why the different exposures affected concrete specimens from the same batch differently. The relatively good relationship between results for large and small beams, both at Treat Island, indicated the two portions of the same batch to be potentially comparable.

We say "potentially." In other publications* we have emphasized the great importance of the moisture condition of the concrete to its resistance to freezing and thawing. It does not seem unreasonable that the two sets of beams sent to Treat Island (large and small) were comparable as to moisture condition. However, the two portions of the batch, one sent to Treat Island and the other tested in the laboratory, could not have been uniform as to moisture.

As a matter of fact, when the *DFE*'s of all fine aggregates are averaged for each coarse aggregate, only the limestone and quartzite show any out-

*Walker, Stanton, "Resistance of Concrete to Freezing and Thawing as Affected by Aggregates," *Circular* No. 26, National Sand and Gravel Assn.; and discussion of "Symposium on Freezing and Thawing Tests of Concrete," *Proceedings*, ASTM, V. 46, 1946, p. 1239.

standing differences in behavior in the laboratory and at Treat Island. In the case of the limestone coarse aggregate, we suggest that the relatively high absorption and the high loss in the soundness test may have combined to account for the uniformly poorer performance in the laboratory than at Treat Island. The high soundness loss probably reflected the presence of the montmorillonite and, to a lesser degree, the high absorption. In the laboratory tests, where the samples were exposed immediately after curing and were kept constantly immersed, the relatively high absorption probably contributed to the poor results because, as the authors say, of absorption of water. At Treat Island the concrete had an opportunity to dry before being exposed and, further, the slow cycle and relatively long exposure to air, including three or four summers, permitted further loss of moisture. Complete resaturation after drying is difficult of attainment.

As to the quartzite, which had a low absorption and high thermal coefficient, it is conceivable that a combination of poor bond, because of the nature of the surface, and high thermal coefficient of the *concrete* may have been a factor. However, some doubt is cast on this hypothesis since the evidence of these characteristics should have exhibited itself as forcibly in the laboratory as in the field, if not more so. Another explanation suggests itself. Leaving out of account the limestone fine aggregate, which was obviously defective (absorption 2.9, $MgSO_4$ loss 20.2), the three crushed fine aggregates had an average *DFE* only three points lower at Treat Island than in the laboratory. The four natural fine aggregates showed an average reduction in *DFE* of 53 points.

What strikes us as being a most significant factor here is that the average cement factor for the crushed fine aggregate concrete was 6.47 while that for the natural sand concrete was 5.57, almost a sack per cu yd less. In terms of absolute volume, the mortar proportions in the first instance were about 1 to 2.4 as compared with 1 to 3.0 in the second. It seems reasonable that the leaner concrete, with leaner mortar, was more vulnerable to chemical attack by the sea water than the richer mixtures, particularly during the nonfreezing periods.

In the case of the granite, the averages for the two exposures were substantially identical. Leaving out of account the unsound limestone fine aggregate, it may be significant that the natural sands (low cement content) gave uniformly lower results at Treat Island, where there was chemical attack, while the richer crushed-fine-aggregate concretes show improvement.

For the uncrushed chert, there was no great difference between the field and the laboratory. The laboratory test was slightly more severe, probably because of the relatively high absorption of the chert. However, this advantage of field storage was less for the natural sand than for the crushed. The explanation given previously appears equally applicable. For the river-terrace gravel, the differences do not appear to be significant, in view of the degree of reproducibility evidenced by duplicate batches for other combinations (39 and 60; 34 and 82; 42 and 50; 69 and 82), and, if the unsound

limestone is omitted, the difference is diminished. Here again, we believe that it must be of significance that such differences as are shown are associated almost exclusively with the lower-cement-content concretes containing natural sands, which, because of their lower cement content, might be expected to be more vulnerable to chemical action.

The crushed chert coarse aggregate averaged consistently higher at Treat Island than at the laboratory. The difference may not be significant in view of the degree of reproducibility evidenced. However, the relatively high absorption of the chert suggests that it may have been more vulnerable in the laboratory, where there was no opportunity for loss in moisture, than the field. Except for the defective limestone, the performance of which has already been discussed, this is the only one of the coarse aggregates for which the relative showing at Treat Island was not significantly better for the richer crushed-fine-aggregate concrete than for the leaner natural-sand concrete. Perhaps the excellent bond of the mortar was a contributing factor and perhaps, also, in this single case, the time was not sufficient to reveal the differences indicated by the other aggregates.

We realize, of course, that these concretes had the same water-cement ratio—and nominally, that was true of the mortars. The explanations based on richness of mixture are in no wise inconsistent with our being among the more aggressive supporters of the water-cement ratio theory. Granted that, in a broad sense, the water-cement ratio establishes the quality of the paste—there were different quantities of this paste used in the different concretes! Abrams, a most thorough and objective investigator, stated the water-cement ratio principle in *Bulletin* No. 1 of the Structural Materials Research Laboratory, December, 1918, as follows: "With given concrete materials and conditions of test the quantity of mixing water used determines the strength of the concrete, so long as the mix is of a workable plasticity." Even when the materials are the same, the water-cement ratio is said only to determine the strength; no one has claimed that a low-cement-content and a high-cement-content concrete are similar with respect to all other characteristics—volume changes due to temperature and moisture, absorption, permeability, and resistance to disintegrating agents.

CLOSURE

This discussion has three objectives. One is to point out the erroneous conclusions that can be reached by adjusting data in accordance with a preconceived idea—and here reference is solely to the DFE versus ΔC relationships based on the adjusted data. The second is to discuss the inadequacy of the data to reveal any differences among aggregates because of the lack of comparability of the concretes and the single-batch results. The third is to suggest that hypotheses to explain differences in behavior between the laboratory and the field may be presented, based on established principles, without resorting to contradictory abstruse reasoning.

By T. F. WILLIS and L. T. MURRAY*

The paper by Kennedy and Mather presents two points of particular interest to the writers: (1) disclosure of the method used by E. J. Callan for adjusting some supplementary data presented in his closure to the discussion of his paper "Thermal Expansion of Aggregates and Concrete Durability;"† and (2) direct information on the relationship between the results of an accelerated laboratory freezing test and those obtained under a specific condition of natural weathering.

First, let us discuss Callan's method of adjusting data, which is described and used by Kennedy and Mather. The writers read Callan's closure to the discussion‡ by Bloem and Walker, R. E. Glover, and Willis and Reagel with a sense of misgiving. Particularly disturbing was the portion in which Callan presented supplementary data which, after an unexplained adjustment, purported to lend credence to his hypothesis of an inverse relationship between *DFE* and ΔC. Before adjustment, neither these supplementary data, nor the data presented by Kennedy and Mather, give support for the hypothesis. To provide such support, any method of adjustment must modify the original data so as to: (a) increase the arithmetical magnitude of the correlation coefficient, and (b) produce a regression line with a negative slope.§ Inspection of Fig. 11 and 12 of the Kennedy-Mather paper will show that both of these stipulations were fulfilled by the dual fine and coarse aggregate adjustments, and our analysis shows that this is equally true after either adjustment alone. But does this fact warrant the conclusion drawn from it?

Adjustment of data is permissible under some circumstances; it serves no useful purpose when applied to data to statistically evaluate a preconceived hypothesis, *unless* the adjustment be carried out by methods that are statistically and hence logically sound. *Under no circumstances should the magnitude of the adjustments be influenced in any way by considerations based on the hypothesis being examined.* With this in mind let us undertake a critical examination of the method of adjustment used. Table A shows Callan's data and is the same as his Table D** except that two columns have been added along the right-hand margin and four rows along the bottom of the table. These added values (according to Kennedy and Mather) were those used by Callan in making his data adjustment.

DATA ADJUSTMENTS FOR FINE AGGREGATES

First, let us examine the adjustments which he considered necessary because

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†Callan, E. J., "Thermal Expansion of Aggregates and Concrete Durability," ACI JOURNAL, Feb. 1952, *Proc.* V. 48, p. 485.

‡Discussion of "Thermal Expansion of Aggregates and Concrete Durability," ACI JOURNAL, Dec. 1952, Part 2, *Proc.* V. 48, p. 504-16. Our discussion will be more readily followed if accompanied by a reading of p. 504-16 of this reference and pp. 157-160 of the Kennedy-Mather paper.

§It is possible for adjustment to be made in such manner that the resulting correlation coefficient would indicate a definite relation between *DFE* and ΔC but produce a regression line of positive slope, which is incongruent with the inverse *DFE*–ΔC relationship postulated by Callan.

**Discussion of "Thermal Expansion of Aggregates and Concrete Durability," ACI JOURNAL, Dec. 1952, Part 2, *Proc.* V. 48, p. 504-16.

TABLE A—FREEZING AND THAWING TEST RESULTS OF 48 CONCRETE COMBINATIONS OF VARIOUS THERMAL PROPERTIES

Top reading in each cell is ΔC of the concrete combination
Lower reading in each cell is *DFE* of the concrete combination

Row No. and coarse aggregate	Fine aggregate	Column No. 1	2	3	4	5	6	7	8	9	10
		Lime-stone	Gran-ite	Sili-ceous river sand	Non-chert sand	Natural sili-ceous sand	Cherty river sand	Quartz-ite	Chert	Aver-age	Rank
	Coeff. of mortar	4.6	5.7	6.3	6.4	7.1	7.3	7.4	7.5	—	—
	Coeff. of aggregate										
1–Limestone	2.3	2.3 50	3.4 30	4.0 28	4.1 33	4.8 23	5.0 56	5.1 34	5.2 28	4.238 35.25	6 6
2–Granite	2.6	2.0 80	3.1 64	3.7 66	3.8 68	4.5 78	4.7 75	4.8 80	4.9 72	3.938 72.88	5 2
3–Nonchert gravel	4.7	−0.1 46	1.0 46	1.6 38	1.7 62	2.4 49	2.6 62	2.7 35	2.8 56	1.838 49.25	4 5
4–Uncrushed chert	5.9	−1.3 74	−0.2 45	0.4 40	0.5 45	1.2 64	1.4 56	1.5 48	1.6 43	0.638 51.88	3 4
5–Crushed chert	5.9	−1.3 88	−0.2 66	0.4 71	0.5 77	1.2 78	1.4 89	1.5 66	1.6 80	0.638 76.88	2 1
6–Quartzite	6.4	−1.8 61	−0.7 59	−0.1 82	0 75	0.7 85	0.9 63	1.0 60	1.1 70	0.138 69.38	1 3
7–Average ΔC 8–Average *DFE*		−0.033 66.50	1.067 51.67	1.667 54.17	1.767 60.00	2.467 62.83	2.667 66.83	2.767 53.83	2.867 58.17	1.904 59.25	– –
9–Rank ΔC 10–Rank *DFE*		1 2	2 8	3 6	4 4	5 3	6 1	7 7	8 5	– –	– –

his experimental concretes contained different lithological types of fine aggregate. These were evolved from the data in the last four rows of the table. Each figure in row 7 is an average of the tabular values of ΔC in the corresponding column. Similarly, each figure in row 8 is an average of the tabular values of *DFE* in the corresponding column. The steps in Callan's adjustment, as described by Kennedy and Mather, were:

(1) Ranking the average values, ΔC in order of ascending magnitude and *DFE* in order of descending magnitude* (shown in rows 9 and 10). Note that in two cases the ΔC rankings are identical with the corresponding *DFE* rankings, while in six cases they are different.

(2) Plotting the average *DFE* values in row 8, against the corresponding average ΔC values in row 7, and drawing a straight line through the two points which had identical *DFE* and ΔC rankings (shown in Fig. D); the writers have designated this as the adjustment datum line.

(3) Scaling the vertical distance from this datum line to each plotted point and then applying the proper scaled magnitude as a correction to each tabular *DFE* in the corresponding column of the table. (For example, in Fig. D the point at the extreme left of the graph is 4.5 units below the datum line, so 4.5 was added to each *DFE* in column 1; similarly the point on the extreme right is 5.0 units above the datum line so 5.0 was subtracted from each *DFE* in column 8; columns 4 and 7 were not changed.)

*The very act, of arbitrarily assigning inverse orders of ranking to the column averages of *DFE* and ΔC, is sufficient to invalidate any inference (about a relation between these variables) which is drawn from adjusted data based in any way on such rankings. From where else, other than the hypothesis itself, could the idea for such an inversion have emanated?

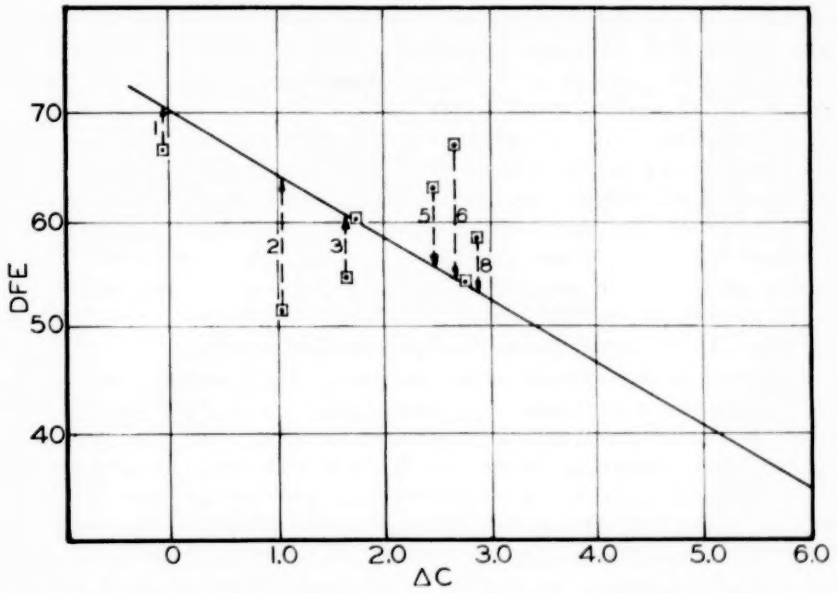

Fig. D—Fine aggregate adjustment. Note: The length of each vertical dotted line is the magnitude of the adjustment Callan made in the *DFE*'s in the indicated column in Table A

Inherent in the method of adjustment is the necessity that the mean of the *DFE*'s in each column of the adjusted data falls on the chosen datum line. As a result the adjusted individual values cluster around the datum line rather than the original regression line; which in turn forces any regression line calculated from the adjusted data to approach the datum line in magnitude and sign of slope, as well as location.*

Since the magnitude of the slope of the datum line controls this characteristic of the regression line of the adjusted data, it can be shown mathematically that: (1) if the slope of the chosen datum line is arithmetically greater than the slope of the regression line of the unadjusted data; and (2) if the "sum of squares" of deviations of the adjusted *DFE*'s from the adjusted mean *DFE* is about the same as, or materially less than, that for the unadjusted data; then the correlation coefficient of the adjusted data will be greater than originally obtained. Both of the above conditions are fulfilled by Callan's method of adjustment. Hence by his choice of a datum line, Callan satisfied requirement (a) stated on p. 172-9. Also, because the sign of the slope of the adjustment datum line largely controls that of the adjusted regression line, fulfillment of requirement (b) is dependent on the choice of a datum line.

As previously described, Callan's datum line for his fine aggregate adjustment was drawn through the only two points where the *direct ranking* of the

*We believe that after reflection this statement will be obvious to the reader so, in the interest of brevity, mathematical proof of the statement is omitted; however, the writers have proved it, as well as the statement in the following sentence, by methods commonly used in the establishment of statistical techniques.

column averages for ΔC coincided with the *inverse ranking* of the column averages for *DFE*. The sign of the slope of a line connecting two such points *must* be negative, and hence in conformance with an inverse relation between ΔC and *DFE*, since it would be impossible for the inverse ranked *DFE*'s to coincide with the corresponding direct ranked ΔC's at *two* points without such points being so related to each other. Hence the fact that the sign of the slope of the datum line agrees with the requirement of the ΔC–*DFE* hypothesis is due only to the experimenter's choice of the two control points. Obviously these points also determine the magnitude of the slope. Therefore, if use of the line as an adjustment datum can be justified, such justification must stem from some unique quality of the two points which determine the line. There appear to be only three such qualities.

First, there is the possibility that coincidence of the *DFE* and ΔC rankings for two sands was so unusual as to indicate some tendency toward systematic order, as opposed to a purely random distribution of the rankings. However, it can be shown by the theorems of probability that, given two piles of randomly assorted chips with the individual chips of each pile being numbered 1, 2, 3, ... 8, if one made eight paired drawings (one drawing of each pair being from each pile), the mathematical expectation is 26.5 percent for obtaining at least two pairs of drawings in which the individual drawings of a pair result in identical numbers. When the mathematical expectancy of an experimental result can be this great, even though obtained from randomly distributed experimental units, the investigator who concluded that such experimental result gave reliable evidence of an ordered relation between the experimental units would indeed be rash; and to use such a conclusion as the foundation for a quantitative adjustment of the data would be throwing caution to the wind. Hence the occurrence of identical *DFE* and ΔC rankings in the case of two fine aggregates provides no logical basis for defining the adjustment datum line.

Second, there is the possibility that those qualities of the sands (such as particle shape and texture), presumed by the authors to be masking the effect of ΔC, were active in the case of six of the sands but absent or passive in the case of the "nonchert" and "quartzite" sands; in other words, that the *DFE*'s of the concretes containing these two sands were determined solely or primarily by ΔC, hence needed no adjustment and could serve as the necessary datum for adjustment of the *DFE*'s of the other concretes. The authors state no basis for such an assumption (and the writers can think of none);* therefore, this possibility must be discarded.

Third, by elimination, we arrive at the conclusion that the only unique feature of the pair of points used to locate the adjustment datum line is the fact that they provide a line having a slope in accord with the hypothesis which the author desired to establish. That this conclusion is not too far fetched will become evident when the coarse aggregate adjustment is analyzed.

*If the authors know of any method of measuring shape and surface texture of aggregate particles, sufficiently quantitative to warrant its use in adjusting statistical data, such information would be of tremendous fundamental value to other investigators.

DATA ADJUSTMENT FOR COARSE AGGREGATE

The coarse aggregate adjustment, according to Kennedy and Mather, was made in a manner analagous to that used for the fine aggregate, using the data in the three columns on the right-hand side of Table A. Examination of the inverse rankings of the average ΔC and the corresponding average *DFE* for each coarse aggregate reveals that in only one instance do the rankings coincide. Since one point cannot uniquely describe a line, the actual location of the datum line used by Callan had to be calculated from his original and adjusted data, together with the fine aggregate adjustments determined from Fig. D. The datum line, so calculated, is the solid line in Fig. E. As shown, it passes through the one point for which the ΔC and *DFE* rankings coincide; is straight in its lower portion; but suddenly deviates from this slope and meanders erratically to its upper end.*

Examination of this line and the plotted points reveals absolutely no basis in the data for the chosen slope and location of the coarse aggregate adjustment datum line. It is obviously not the "least squares" line through the points. The fact that one end of it is anchored by the single point for which the ΔC and *DFE* rankings coincided has even less significance than in the case of the fine aggregate adjustment. If the rankings were sets of random numbers the mathematical expectancy for the occurrence of at

*Probably this is due to computational errors by the original author. Had he used the dotted extension of the line for adjusting the average *DFE*'s of uncrushed chert, crushed chert, and quartzite he would have obtained a correlation coefficient for his adjusted data even larger than 0.847.

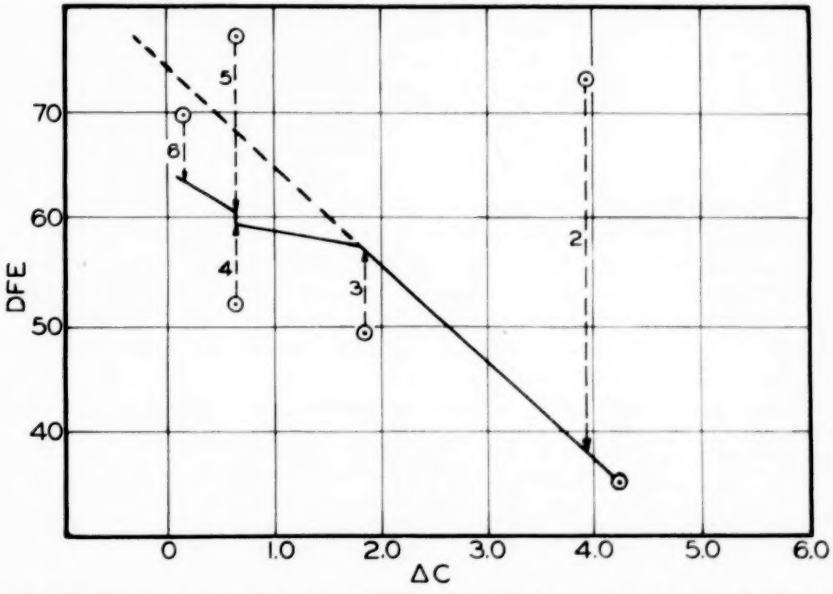

Fig. E—Coarse aggregate adjustment. Note: The length of each vertical dotted line is the magnitude of the adjustment Callan made in the *DFE*'s (after fine aggregate adjustment) in the indicated row of Table A

least one pair of identical numbers would be well above 50 percent; in other words, even with random numbers it would be more unusual that none of the rankings should pair up than for one or more to do so. Therefore the occurrence of the single instance of paired rankings gives absolutely no evidence of an ordered relation between *DFE* and ΔC. Furthermore, there is no indication in the paper that the authors possessed any knowledge (about the effects of surface texture and particle shape of the coarse aggregates on *DFE*) sufficiently quantitative to warrant definition of an adjustment datum line.

Again, just as for the fine aggregate adjustment, we are forced to the conclusion that the only basis for choice of the coarse aggregate adjustment line* was that the chosen line provided adjusted data which conformed more closely to the hypothesis than did the original data.

GENERAL COMMENT ON THE HYPOTHESIS OF AN INVERSE RELATION BETWEEN *DFE* AND ΔC

It then seems apparent that the magnitudes of the *DFE* adjustments for both fine and coarse aggregate, and hence the correlation coefficient and regression line of the adjusted data, are not only influenced, but largely controlled by considerations which emanated solely from the hypothesis itself. Hence, the adjusted data, and of course the regression analysis performed thereon, are meaningless.† From our study, we conclude that Callan in effect (1) adopted an hypothesis; (2) by his method of adjustment forced the experimental data to conform to the hypothesis; and (3) finally concluded, because the adjusted data (having no alternative) showed a regression ilne of the proper slope and a high correlation between the variables of the hypothesis, that this fact supported the validity of the hypothesis.‡ Surely *no* conclusion should be considered as being validated in even a minute degree by such circuitous reasoning.

Kennedy and Mather subjected their experimental data to the same sort of adjustment, and these data, *after adjustment*, also show a highly Significant correlation between *DFE* and ΔC. Kennedy and Mather were apparently somewhat skeptical of the validity of the pseudo-correlation for they state: "It would appear from these data that there is a correlation between *DFE* and ΔC, but that the correlation is probably usually of lesser importance than other characteristics of the concrete." Nevertheless, the fact that they made the adjustment and published the graphs lends weight to the exist-

*The coarse aggregate adjustment has the major effect in improving the pseudo-correlation. This is in the right direction. Considering Table A as a two-way classification, an analysis of the variance of the *DFE*'s (disregarding ΔC) shows that the effect of varying type of fine aggregates was barely Significant; whereas, the effect of changing coarse aggregates was highly Significant. However, even such an analysis provides no basis for adjustment of the data, since the effects due to particle shape, texture, or what-not are inextricably confounded with the effect due to ΔC, *if there is such;* neither does it provide justification for Callan's method for determining the magnitude of the adjustments.

†There is a statistically and logically sound method of adjusting data to eliminate the effect of nonquantitative variable factors on a regression. For the data under discussion such factors would be the coarse and fine aggregates; the effects could be any which would result from varying these factors. The writers performed such an adjustment on the data of Table A. The resulting correlation coefficient was slightly greater than Callan's original value of 0.312 but the difference was insignificant at even the 50 percent probability level.

‡Using a similar method of adjustment and reasoning but different initial postulates, one could arrive at the conclusion that the *DFE*–ΔC relation was direct instead of inverse as assumed by Callan, or that there was zero correlation between these factors.

ence of a DFE–ΔC relationship. When one considers the method used for the data adjustment—together with the implications of the unadjusted data*—there is no evidence, in either Callan's supplementary data or the data of the paper by Kennedy and Mather, for the existence of a DFE–ΔC relationship. It therefore seems to us that a more objective appraisal of the latters' data is contained in the following statement: "The data of the experiment being reported provide no evidence to support the existence of a DFE–ΔC relationship; if ΔC had *any* effect on DFE, such effect was effectually and completely masked by the effects of other characteristics of the concrete."

In the light of all the published information about the "thermal incompatibility hypothesis," it is our opinion that it should be laid back on the shelf along with other, superficially plausible but presently unestablished, hypotheses about concrete; not forgotten, but considered as at least temporarily passé until and unless someone proves it through the medium of an experiment properly designed for the express purpose. As we interpret the literature, the hypothesis has not been successful even as a speculative explanation for unexpected behavior of experimental concretes; and certainly, up to the present, there has been offered no evidence which sufficiently validates the hypothesis to warrant its consideration in the routine selection of aggregate combinations for use in actual structures.

LABORATORY FREEZING TESTS VERSUS NATURAL WEATHERING

Thanks are due Kennedy and Mather for their presentation of data on one phase of concrete durability testing on which factual information has been meager; this is the degree to which laboratory durability tests are useful in predicting the durability of concrete under field exposure. In the instance they examined, the accelerated laboratory tests failed completely to predict the relative durability of the various concretes under outdoor exposure. Admittedly the outdoor exposure they used is out of the ordinary. However, the writers have the feeling that similar experiments with other more common field exposures will disclose that some laboratory freezing procedures produce test results which are of doubtful utility as estimates of the durability of concrete under actual weathering, and that more agencies should be actively investigating the possibility.

AUTHORS' CLOSURE

The authors thank Messrs. Walker and Bloem, and Willis and Murray for their discussions, both of which are principally concerned with a procedure for adjusting data presented by E. J. Callan in 1952.[1] Since our description of the mechanics and concept of Callan's adjustment, as written, was not clear to the discussors, their correct and extended discussions of it are desirable contributions. Both pairs of discussors appear to have neglected

*Note that the regression lines for the unadjusted Kennedy-Mather data had positive slopes, indicating (if anything) a direct—rather than an inverse—relation between DFE and ΔC.

Callan's statement of the limited intent and application of the adjustment: "These results should be construed only as a general indication that the *DFE*–ΔC relationship holds for these combinations, although hidden in the actual results due to the effects of other factors affecting the over-all concrete quality."[2]

We regret that Walker and Bloem regard us as intending to support, to a degree not justified by the evidence, the idea that "the greater the difference between the thermal coefficients of the coarse aggregate and the mortar, the less able is the concrete to resist temperature changes and particularly freezing and thawing."[3] Later in this discussion we will review the evidence. We stated[4] that "It should be noted from both large and small beams that other characteristics of aggregates and concretes were of sufficient magnitude to completely reverse the apparent correlation between *DFE* and differences in coefficient of expansion for the 48 aggregate combinations in this series. It would also appear that each aggregate combination should be evaluated through tests made on concrete containing that combination and no combination should be given a clean bill of health or condemned because of any specific value for ΔC." Later we wrote:[5] "Little correlation between ΔC and durability factor was found, because other properties of the aggregate exerted more important influences." We believed that the passages just quoted should have made it clear that we held no brief for accepting or rejecting coarse aggregates only because they differ appreciably in thermal coefficient of expansion from the mortar to surround them.

As Walker and Bloem indicate, we found[6] that there was no correlation between *DFE*'s of small beams tested in the laboratory and in the field, or between small laboratory and large field beams, but good correlation between the *DFE*'s of the two sizes of beams tested in the field. We tried to evaluate the features of the concrete and the properties of the aggregates that seemed likely to be causally related to differences in results in the two exposures. We believed that it would be apparent that we were more impressed by the lack of correlation in the raw data than by the adjusted data. If we had accepted the correlations in the adjusted data as heartily as Walker and Bloem appear to assume, no explanations of differences in results would have been required. The space devoted in the paper to the results of the examination indicates the importance we attached to the differences in results in the two exposures, and to the departures of actual results from results expected, if differences in thermal coefficient between coarse aggregate and mortar had had a major influence on resistance to freezing and thawing and natural weathering.

After burying Callan's adjustment at the crossroads with a stake through its heart, Walker and Bloem present an eloquent argument in favor of proportioning concrete mixtures for durability testing with constant cement factor rather than constant water-cement ratio. We believe both methods are defensible and the choice between them is largely subjective. The consequences of selecting constant water-cement ratio are that aggregate combi-

nations with more desirable particle shape will have lower cement factors, and aggregate combinations with less desirable particle shape will have higher cement factors with some advantage in quality of cement paste. The consequences of selecting constant cement factor are that combinations with more desirable particle shape will have lower water-cement ratios with some advantage in quality of cement paste, while combinations with less desirable particle shape will have higher water-cement ratios. If the water-cement ratio is fixed, smooth surfaced, equidimensional rounded gravel and sand combinations require less cement for a given slump; if the cement factor is fixed, such combinations require less water. Either choice is in a sense discriminatory; unless it is possible to test every combination in concrete proportioned in both ways, definitions of what Walker and Bloem wisely call "practical comparability" will be in part subjective.

Walker and Bloem emphasize the difference that we had noted[7] between the constantly wet condition of the beams tested in the accelerated freezing-and-thawing apparatus and the fluctuating, generally drier, condition of the beams at Treat Island. In the Treat Island exposure, they regard the 3½ x 4½ x 16-in. beams as comparable in moisture condition to the 6 x 6 x 30-in. beams. Since each small beam has 1.12 sq in. of surface per cu in. of volume while each large beam has 0.73 sq in. of surface per cu in. of volume, it seems more likely that the fluctuations of moisture condition in the large beams ran parallel with those of the small beams but were less extreme. Wetting and drying in each size of beam in the Treat Island exposure complicates and probably increases moisture movement in the concrete, and the moving water contains more dissolved material than the water in the specimen containers of the laboratory apparatus.

THE HYPOTHESIS OF THE INVERSE RELATION BETWEEN *DFE* AND ΔC

It is our intention here: (1) to review some of the history of the concept that an inverse relation exists between *DFE* and ΔC, and (2) to draw attention to the specialized circumstances in which we believe it possible for differences between thermal coefficients of coarse aggregate and mortar recognizably to influence resistance of concrete to freezing and thawing.

In 1939 Hornibrook, and Willis and De Reus, briefly discussed[8] whether resistance of concrete to freezing and thawing might be adversely affected by large differences in coefficient of expansion between the aggregate and the "cement matrix." J. E. Pearson's "A Concrete Failure Attributed to Aggregate of Low Thermal Coefficient"[9] is the reference most frequently cited in discussions of the effects of thermal coefficients of concrete constituents; it describes a failure of cast stone that was attributed in part to stresses set up by differences in thermal expansion between the aggregate and the cement matrix. Pearson called attention to several factors in the exposure and in the concrete that were either unusual or inherently disadvantageous; these are worth recalling since this is the best documented instance of a failure in service that has been attributed to thermal expansion. First, the

steps were unusually dense concrete cast in midsummer, so that internal stresses in each step presumably increased as the ambient temperature decreased. Second, the drainage of the steps and their mortar bed was poor, so that the steps were wet when freezing and thawing occurred. Third, the outer surfaces of the steps were dressed to expose the aggregate. Fourth, the aggregate was fairly coarse-grained dolomitic marble; crushing such a rock would open the cleavage planes of some of the dolomite crystals; exposed aggregate on the surfaces of the steps probably had some slightly opened cleavage planes arranged so that water standing on the surface of the steps could enter them. These features place the instance outside of the range of most pavement or structural concrete.

The test specimens that Pearson made to investigate his hypothesis contained Ottawa sand and four different coarse aggregates of low thermal coefficient—two marbles, calcite, and Vycor glass. The differences between the thermal coefficient of the mortar (6.5×10^{-6} approximately) and those of the aggregates (-1.2 to $+2.5 \times 10^{-6}$ in the range from -25 to $+75$ F) were between 4 and 8×10^{-6} per deg F. All of the specimens expanded more than 0.1 percent in 40 cycles of laboratory freezing and thawing in water, but the glass and calcite had smoother particle surfaces and concrete made with them failed more quickly than that made with the two marbles. The test specimens had the following unusual features: (1) fine aggregate was Ottawa sand having a grading different from that normally used in concrete, (2) coarse aggregates were of types not normally used in concrete, and (3) the coarse aggregate particles had unusually smooth surfaces.

Some of the discussors of Pearson's paper were thinking in terms of differences in thermal coefficients between aggregates and other constitutents of the concrete, as the quotations below illustrate.

"Mr. Pearson has now commendably pointed out another cause of disintegration, namely, that which results when, *under certain conditions of exposure*, the thermal expansivity of the aggregate is incompatible with that of the associated materials."[10]

"High internal stresses must occur when materials with widely different thermal expansions are yoked together and then subjected to alternations in temperature, and it is axiomatic that such a condition would induce strains *contributing* to failure."[11]

"It is very evident that if the coefficient of the aggregate be very greatly different than that of the cement paste, whether higher or lower, it would *tend* to set up destructive stresses in the concrete in the first case at the higher temperatures, in the second, at the lower."[12]

(The italics are by Kennedy and Mather, not the authors quoted.)

Reagel and Willis[13] recommended tests to separate the effects of differential thermal expansion from the effects of wet freezing and thawing; Pearson[14] took up this pertinent suggestion and made tests of bars air-dried for 8 weeks and then frozen and thawed in air. Three coarse aggregates were tested in concrete mixtures of constant water-cement ratio with Ottawa sand fine aggregate; the coarse aggregates were Maryland marble, Vycor glass, and Ohio silica pebbles. After 50 cycles, the bars with marble and glass coarse aggregates had lost 40 to 50 percent of their original modulus of elasticity while the bars with silica pebbles had lost less than 10 percent of their original

modulus. The cement contents of the three mixtures were 5.64 bags per cu yd in the glass concrete, 6.15 in the silica pebble concrete, and 6.57 in the marble concrete.

Callan[15] described an investigation intended to explain some apparently anomalous results of laboratory freezing and thawing tests. He reviewed the available test data on the concretes and aggregates involved and conducted correlation analyses to obtain measures of the relationship between durability factor of the concretes and difference in thermal coefficients of the coarse aggregates and mortars, both with and without simultaneous consideration of the absorption of the coarse aggregates. The results of his analyses for 78 combinations of 22 coarse and 29 fine aggregates may be summarized as follows:

(a) Multiple correlation coefficient of *DFE* with ΔC and absorption of coarse aggregate: $R = 0.719$
(b) Partial correlation coefficient of *DFE* with ΔC, absorption eliminated: $r_{DFE,\ \Delta C\ Abs} = 0.5976$
(c) Simple correlation coefficient of *DFE* with ΔC, disregarding absorption: $r = -0.6165$

It should be emphasized that all that a correlation coefficient provides is a measure of the degree of relationship between variables; it does not provide information about the reasons for the relationship nor about cause and effect.

By squaring a correlation coefficient one obtains a coefficient of determination, a measure of the extent to which the variance of the dependent variable is determined by the variance of the independent variable, if it may be assumed that the two variables are causally related.[16] If it is assumed for the moment that durability factor is causally related to ΔC and to absorption of coarse aggregate, and coefficients of determination are calculated from the data of Callan's paper, they are:

(a) Coefficient of multiple determination of *DFE* by ΔC and absorption of coarse aggregate $= 0.719^2 = 0.517$, or 52 percent
(b) Coefficient of determination of *DFE* by ΔC alone $= -0.6165^2 = 0.380$ or 38 percent

Thus, even if it is conceded that *DFE* is causally related to ΔC and absorption, only 52 percent of the variation in *DFE* in the tests that Callan reported may be explained by variation in ΔC and absorption; and for the simple correlation between *DFE* and ΔC only 38 percent of the variation in *DFE* in the data of Callan's paper may be explained by variation in ΔC, leaving 62 percent still to be explained.

Such was the situation in laboratory tests in an environment permitting little change in the nearly saturated condition of the concretes, among a series of aggregate combinations of restricted lithologic range and consequently of restricted range in thermal, mechanical, and chemical properties. With correlations no more intense than those found in Callan's data, departures from either the test environment or the restricted lithologic range may be expected to conceal or destroy the relation between durability factor and difference in thermal coefficients. The tests that we described departed from the restricted

environment by including the Treat Island exposure, and departed from the restricted lithologic range by including aggregates of varying capacities for cement-aggregate reaction, varying moduli of elasticity, and in one case at least, swelling clay. Since it seems inescapable that the durability or non-durability of concrete is almost always a result of combined causes, it is not surprising that the relation did not emerge.

We wish to correct an error on p. 152 of the paper. In the last sentence of the last paragraph of the section on test results, the first portion reading "Except for limestone-coarse-aggregate concrete, where the average *DFE* of large beams was slightly lower than that of small field beams" should be deleted and the sentence should read: "The *DFE* was higher for large beams than for small beams at Treat Island."

Attention may also be called to the fact that the coarse aggregate referred to elsewhere in our paper as "river-terrace gravel" is designated as "non-chert gravel" in Fig. 11 and 12 (pp. 157 and 158), which were reproduced from Callan's closure.[1]

REFERENCES

1. Callan, E. J., closure to "Thermal Expansion of Aggregates and Concrete Durability," ACI JOURNAL, Dec. 1952, Part 2, *Proc.* V. 48, p. 504-11.

2. *Op cit.*, p. 504-16.

3. Discussion by Walker and Bloem.

4. Kennedy, T. B., and Mather, K., "Correlation Between Laboratory Accelerated Freezing and Thawing and Weathering at Treat Island, Maine," ACI JOURNAL, Oct. 1953, *Proc.* V. 50, p. 160.

5. *Op. cit.*, p. 161.

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Discussion of a paper by T. Y. Lin and A. C. Scordelis:

Selection and Design of Prestressed Concrete Beam Sections*

By PANOS D. MOLIOTIS†

In the interesting paper by Messrs. Lin and Scordelis, the tables for the quick calculation of prestressed concrete beams sections, in addition to the necessary elements of sections and factors for interior stresses, use the formula

$$\frac{Q_2}{Q_1} = \frac{F_o}{F} \times \frac{e-k_b}{e+k_t} \leq \frac{M_G}{M_T}$$

which is the criterion for the appropriate selection of a section. With the aid of this formula and trial sections, we can select the type and dimensions of a section in which the developed interior stresses come near the limit of permissible stresses.

In the following discussion the factor Q_2/Q_1, symbolized by V, is given independently of the ratio F_o/F, *i.e.*, $V = (e - k_b)/(e + k_t)$ and is extended to the case of a tensile stress f_{to} at the top fiber. We think that the factor V should be independent of F_o/F as it is possible that this ratio will vary in different countries. Also suggested as a criterion is the factor $\mu = c_b/h\ (1 + e/k_t)$. Combining V and μ we can select a section using the tables without any trial sections.

The formulas have been derived on the assumption that at the bottom of the section the stress is zero for the final loading.

Notations

In addition to the notations used in the original paper, the following have been added:

e = distance of the center of the prestressing force from the lower limit of the kern of the section

$n = F_o/F$

f_{to} = concrete stress at the top fiber of the section at transfer

$$V = \frac{e-k_b}{e+k_t}$$

$$\mu = \frac{c_b}{h}\left(1 + \frac{e}{k_t}\right)$$

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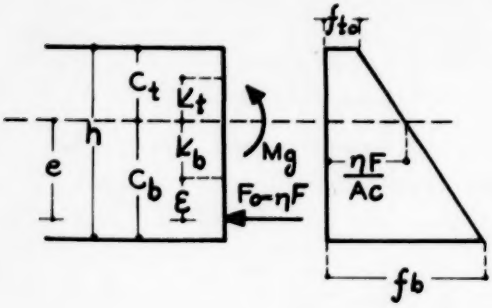


Fig. A—Stresses at transfer

STRESSES AT TRANSFER

The total bending moment acting on the section (Fig. A) is

$$M = M_G - nF\,(k_b + e)$$

The stress f_{to} is given by

$$f_{to} = \frac{nF}{A_c} + \frac{M_G\,c_t}{I} - \frac{nF\,(k_b + e)\,c_t}{I}$$

But $$\frac{nF}{A_c} - \frac{nF\,k_b\,c_t}{I} = 0$$

From the above two equations we obtain

$$nF\,e = M_G - f_{to}\frac{I}{c_t} \qquad (1)$$

The stresses f_{to} and f_b are related to each other in the equation

$$f_{to}\frac{c_b}{h} = \frac{nF}{A_c} - \frac{c_t}{h}f_b \qquad (2)$$

FINAL STRESSES

The total bending moment acting on the section (Fig. B) is

$$M = M_T - F\,(k_b + e)$$

The stress f_t is given by

$$f_t = \frac{F}{A_c} + \frac{M_T\,c_t}{I} - \frac{F\,(k_b + e)\,c_t}{I}$$

But $$\frac{F}{A_c} - \frac{F\,k_b\,c_t}{I} = 0$$

Combining the above equations we obtain

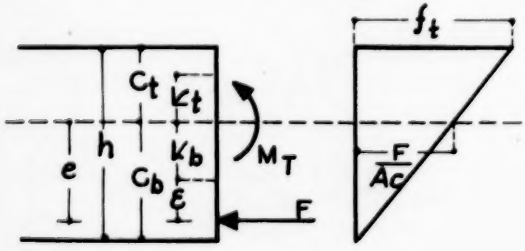


Fig. B—Final stresses

$$f_t \frac{I}{c_t} = M_T - F\,\epsilon \quad (3)$$

Prestressing force F is given by the equation

$$F = \frac{c_b}{h} A_c f_t \quad (4)$$

DESIGN FORMULAS

If in Eq. (3) we replace f_t by its value from Eq. (4) and have in mind that $1/A_c = (k_b\, c_t)/I$, $\epsilon = e - k_b$, and $k_b/k_t = c_b/c_t$, we obtain

$$M_T = F\,(e + k_t) \quad (5)$$

Combining Eq. (4) and (5) we obtain

$$\frac{M_T}{f_t} = \frac{c_b}{h} A_c\,(e + k_t) \quad (6)$$

With the aid of Eq. (6) and using tables, we compute the dimensions of the section. Prestressing force is computed from Eq. (4). Combining Eq. (2) and (4) we obtain

$$f_{to} = nf_t - \frac{c_t}{c_b} f_b \quad (7)$$

From Eq. (7) we compute the stress f_{to}. If in Eq. (1) and (3) we replace the product $F\epsilon$ and have in mind Eq. (7) we obtain

$$f_b = \frac{nM_T - M_G}{A_c\, k_t} \quad (8)$$

From Eq. (8) we compute the stress f_b.

CRITERIA FOR SELECTING SECTIONS

Eq. (1) one can be rewritten

$$nF\,(e - k_b) = M_G - f_{to}\, A_c\, k_b \quad (9')$$

Eq. (8) can be rewritten

$$\frac{c_t}{c_b} k_b\, A_c = \frac{nM_T - M_G}{f_b} \quad (8')$$

If in Eq. (9′) we replace $A_c\, k_b$ by its value and have in mind Eq. (7), then Eq. (9′) becomes

$$nF\,(e - k_b) = M_G - \frac{f_{to}}{nf_t - f_{to}}\,(nM_T - M_G) \quad (9'')$$

If we divide Eq. (9″) by Eq. (5) we obtain

$$V = \frac{e - k_b}{e + k_t} = \frac{1}{n} \times \frac{M_G}{M_T} - \frac{f_{to}}{nf_t - f_{to}}\left(1 - \frac{1}{n}\frac{M_G}{M_T}\right) \quad (9)$$

If stress f_{to} is zero or compressive we must have

$$V \leq \frac{1}{n} \times \frac{M_G}{M_T} \quad (10)$$

In case some tensile stress is permitted we must have

$$V \leq \frac{1}{n} \times \frac{M_G}{M_T} - \frac{f_{to}}{nf_t - f_{to}}\left(1 - \frac{1}{n} \times \frac{M_G}{M_T}\right) \quad (10')$$

(In the above formulas compressive stresses have positive values and tensile ones negative.)

If we divide Eq. (6) by Eq. (8) we obtain

$$\mu = \frac{c_b}{h}\left(1 + \frac{e}{k_t}\right) = \frac{f_b}{f_t} \times \frac{M_T}{nM_T - M_G} \quad \ldots\ldots (11)$$

For stress f_t equal to the permissible one and f_b equal or smaller than the permissible one we have

$$\mu \leqq \frac{f_b}{f_t} \times \frac{M_T}{nM_T - M_G} \quad \ldots\ldots (12)$$

With the aid of the parameters V and μ and tables like those proposed by Messrs. Lin and Scordelis we can select a section which is stressed at the permissible stresses f_t and f_b and f_{to} can be a small compressive or tensile stress or zero.

EXAMPLES

The following examples utilize the tables of the paper.

Example 1

Given—M_T = 725 ft-kips
M_G = 217 ft-kips
f_t = 1.4 kips per sq in.
f_b = 2.0 kips per sq in.

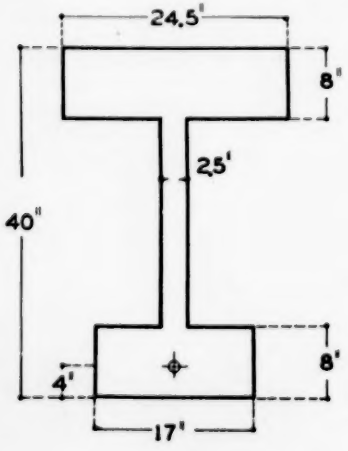
$1/n$ = 0.85
n = 1.175
d/h = 0.9

Solution

$$V \leqq 0.85 \times \frac{217}{725} \leqq 0.255$$

$$\mu \leqq \frac{2.0}{1.4} \times \frac{725}{1.175 \times 725 - 217} = 1.63$$

We choose an I-section (4-b in Table 4) with: $\mu = 0.560\ (1 + (0.460/0.248)) = 1.59$; $A_c = 0.40\,bh$; $c_b = 0.560\,h$; $k_t = 0.248\,h$; $e = 0.460\,h$; and $(c_b/h)\,A_c\,(e + k_t) = 0.560 \times 0.40 \times (0.460 + 0.248)\,bh^2 = 0.158\,bh^2$. Through Eq. (6) we get: $(725 \times 12)/1.4 = 0.158\,bh^2$, and for $h = 40$ in. we get $b = 24.5$ in. (Fig. C).



Thus we have:

$A_c = 0.40 \times 24.5 \times 40 = 392$ sq in.
$F = 0.560 \times 392 \times 1.4 = 308$ kips

$$f_b = \frac{(1.175 \times 725 - 217) \times 12}{392 \times 0.248 \times 40} =$$

1.95 kips per sq in.

$$f_{to} = 1.175 \times 1.4 - \frac{0.440}{0.560} \times 1.95 =$$

0.11 kips per sq in.

Fig. C—I-section for example 1

Example 2

Given—M_T = 725 ft-kips
M_G = 435 ft-kips
f_t = 1.4 kips per sq in.
f_b = 2.0 kips per sq in.
$1/n$ = 0.85
n = 1.175
d/h = 0.9

Solution

$$V \leqq 0.85 \times \frac{435}{725} \leqq 0.510$$

$$\mu \leqq \frac{2.0}{1.4} \times \frac{725}{1.175 \times 725 - 435} \leqq 2.50$$

We choose an I-section (3-i in Table 3) with: $\mu = 0.592\left(1 + \frac{0.492}{0.164}\right) = 2.37$; $A_c = 0.57\ bh$; $c_b = 0.592\ h$; $k_t = 0.164\ h$; $e = 0.492\ h$; and $(c_b/h)\ A_c\ (e + k_t) = 0.592 \times 0.57 \times (0.492 + 0.164)\ bh^2 = 0.222\ bh^2$. Through Eq. (6) we get: $(725 \times 12)/1.4 = 0.222\ bh^2$, and for h = 40 in. we get b = 17.5 in. (Fig. D).

Thus we have:

$A_c = 0.57 \times 40 \times 17.5 = 400$ sq in.

$F = 0.592 \times 400 \times 1.4 = 330$ kips

$$f_b = \frac{(1.175 \times 725 - 435) \times 12}{400 \times 0.164 \times 40} =$$

1.9 kips per sq in.

$$f_{to} = 1.175 \times 1.4 - \frac{0.408}{0.592} \times 1.9 =$$

0.25 kips per sq in.

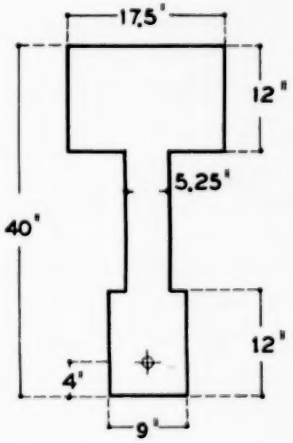


Fig. D—I-section for example 2

Example 3

Given—M_T = 725 ft-kips
M_G = 217 ft-kips
f_t = 1.4 kips per sq in.
f_b = 2.0 kips per sq in.
f_{to} = −0.14 kips per sq in.
$1/n$ = 0.85
n = 1.175

Solution

$$V \leqq 0.85 \times \frac{217}{725} + \frac{0.14}{1.175 \times 1.4 - 0.14}\left(1 - 0.85 \times \frac{217}{725}\right) \leqq 0.324$$

$$\mu \leqq \frac{2.0}{1.4} \times \frac{725}{1.175 \times 725 - 217} = 1.63$$

We choose an I-section (4-g in Table 4) with: $\mu = 0.534\left(1 + \frac{0.434}{0.220}\right) = 1.59$; $A_c = 0.41\ bh$; $c_b = 0.534\ h$; $k_t = 0.220$ h; $e = 0.434\ h$; and $(c_b/h)\ A_c\ (e + k_t) = 0.534 \times 0.41 \times (0.434$

$+ 0.220)\, bh^2 = 0.143\, bh^3$. From Eq. (6) we get: $(725 \times 12)/1.4 = 0.143\, bh^2$, and for $h = 40$ in. we get $b = 27$ in. (Fig. E).

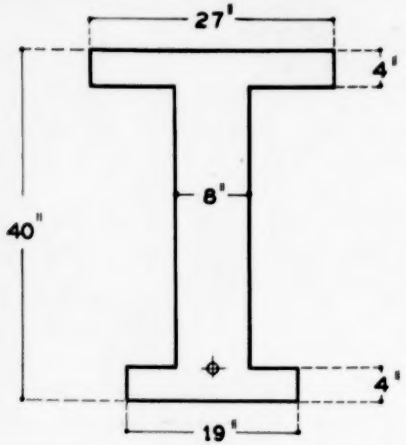


Fig. E—I-section for example 3

Thus we have:

$A_c = 0.41 \times 40 \times 27 = 443$ sq in.

$F = 0.534 \times 443 \times 1.4 = 330$ kips

$$f_b = \frac{(1.175 \times 725 - 217) \times 12}{443 \times 0.220 \times 40} =$$

1.95 kips per sq in.

$$f_{to} = 1.175 \times 1.4 - \frac{0.466}{0.534} \times 1.95 =$$

− 0.05 kips per sq. in.

CONCLUSIONS

The formulas of this discussion can be used easily for the determination of the point from which the prestress force can be decreased, due to the upward turning of the cables. With the aid of Eq. (1) with a constant prestress force F and a constant section we compute the variation of the stresses f_{to} with the aid of

$$\Delta f_{to} = \frac{\Delta M_G}{A_c k_b} \qquad (13)$$

Similarly with the aid of Eq. (5) for a constant section and a variable moment M_T we compute the variation of the prestress force F with the aid of

$$\Delta F = \frac{\Delta M_T}{e_t k_t} \qquad (14)$$

If we prepare tables for different types of sections similar to those of Messrs. Lin and Scordelis containing the factors μ and V, we can select sections such that the stresses can approach the permissible limits of concrete stresses. It results in a complete utilization of the concrete section so far as concrete is concerned.

Discussion of a paper by J. Calleja:

Determination of Setting and Hardening Time of High-Alumina Cements by Electrical Resistance Techniques*

By E. H. WATERS and AUTHOR

By E. H. WATERS†

Dr. Calleja's paper is interesting not only because of the information it contains about the setting of high-alumina cements but also because of the light it throws on the results obtained in his earlier investigation of the use of electrical resistance techniques for the study of the setting and hardening of portland cements.[1]

In the discussion of that paper[2] it was pointed out that the differences in time between the occurrence of the maxima of the temperature curves and the minima of the resistance curves were such that it seemed rash to claim (as Dr. Calleja had done) that they were wholly due to the heat capacity of the thermometer and associated apparatus. It was suggested that further investigation was necessary on this point because it was felt that other factors besides heat capacity were probably responsible in part for the observed differences. The data presented by Dr. Calleja on high-alumina cements now provide some evidence in favor of that idea.

In his experiments with portland cements Dr. Calleja found the differences between the characteristic points of the resistance and temperature curves small enough, relative to the total setting time, to enable them to be ignored for practical purposes and so permit resistance measurements to be used to determine the time of set. The results of the investigation now reported on high-alumina cements however show such a wide divergence between the resistance minima and the results obtained by the more conventional methods that resistance measurements become useless as a means of measuring the final set. To explain the difference in behavior he has observed in his resistance measurements on the two types of cement, Dr. Calleja commences by saying that "the setting and hardening of hydraulic cements are usually considered as successive rather than simultaneous phenomena." He then goes on to postulate that high-alumina cement differs from portland cement (and presumably, from the above quoted statement, from most other hydraulic materials) in having the hardening and setting processes overlapping and on

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this basis explains why the second minimum of the resistance time curve appears so much earlier (relative to the temperature-time curve) for high-alumina cement than for portland cement.

To regard the setting and hardening of a cement as successive rather than simultaneous phenomena is surely an over simplification. Setting and hardening must always overlap, otherwise the setting reaction would not be so called. It is the appearance of a certain amount of hardening of the paste which makes us say that it is setting. Most of the methods of determining setting time depend on measuring the hardness of the paste (Vicat's needle, Gillmore's needle, etc.). The temperature maximum is used as a measure of setting time (and the reaction which produces it called the setting reaction) only because it occurs when the paste has reached a certain degree of hardness.

Since there is no sharp end to the setting process of any hydraulic material and consequently all the various methods of measuring setting time are arbitrary, no two can be expected to give exactly the same answer. The temperature maximum for example will not coincide with the end of the set as defined in the first paragraph of the "Discussion" (p. 253) except in an almost adiabatic system. In such a system the curve will be so flat-topped that the position of the maximum will be difficult to define with any precision. The usual temperature-time curves obtained are of the types shown in Fig. 2 and 3 with a fairly sharply defined maximum showing that the system departs considerably from the adiabatic condition. Not only the sharpness of the peak but also the time of its occurrence will be affected to some degree by the extent of this departure. To define the end of set as the moment when the temperature reaches its maximum value is thus only slightly less arbitrary than defining it by the mechanical behavior of the cement paste under the impact of a loaded needle.

It is thus impossible to separate in time the setting and hardening of any hydraulic cement. The extent to which hardening has taken place before the arbitrarily defined end of set and the extent to which the heat-producing chemical reactions continue after hardening is detectable will vary with different cements, but some overlap will always be found. That is, the tendency for the resistance of the paste to increase, because of the decrease of ionic mobility accompanying hardening, will begin before the rise in temperature, with its tendency to reduce resistance, has ended. Hence the minimum in the resistance-time curve will, in general, occur before the maximum of the temperature-time curve. With portland cements the difference is fairly small as Dr. Calleja's earlier results show, but with high-alumina cements it is much greater as the results under discussion have shown. In general, the form of the resistance-time curve will depend then on the extent to which the exothermic chemical reaction must proceed before the resultant hardening causes appreciable interference with the mobility of the ions of the solution and so increases the resistance of the paste. It is conceivable that with some materials there would be no minimum at all; if, for example, a very small amount of reaction was sufficient to produce a great deal of mechanical

hardening the resistance could be expected to increase continuously. The intermediate case where the two effects are in balance for some time has frequently been observed by the Division of Building Research with plaster of paris where the resistance-time curve usually shows only a plateau (occasionally a very shallow minimum) before the final steady rise in resistance.

The differences observed between the changes in electrical resistance of portland and high-alumina cements thus reflect a difference in the setting behavior of these cements not in kind but in degree only.

Dr. Calleja's diagram in Fig. 3 and his accompanying explanation are an over simplification—setting and hardening always overlap (otherwise the setting reaction would not be so called)—the question is solely the degree to which this occurs.

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AUTHOR'S CLOSURE

Though there is copious literature on the hydration process of portland cement, relatively little has been written on the hydration of high-alumina cement. In our present state of knowledge, the idea of a partial superposition of the setting and hardening processes in high-alumina cement, to account for the differences exhibited by these in respect to portland cement, should be viewed only as a working hypothesis. In this sense the viewpoint of Mr. Waters is highly interesting either as an alternative, or even an opposite hypothesis.

I shall attempt to give below the facts and ideas which have led me to assume that in the case of portland cement the setting and hardening phenomena should be considered to occur consecutively rather than simultaneously. These, however, are relative terms, both total superposition and exact succession being limiting cases, which may not arise in practice.

It is true, as Mr. Waters says, that "it is the appearance of a certain amount of hardening of the paste which makes us say that it is setting," and hence "the majority of methods to determine setting are based on the measure of hardness of the paste." If there was no sharp end to the setting process, then, evidently, all the methods referred to before would be arbitrary. On the one hand, not two of them would yield the same result, and on the other, the results according to the same method, would vary with changing experimental conditions.

But on analyzing the well defined minima exhibited by the *R-t* curves, and the well defined point which separates the two sections of the dielectric constant-time curves (in the case of portland cement pastes),[1] we were led

to consider the possibility of a discontinuity in the hydration phenomena. Or, what is the same thing, a sequence in the setting and hardening phenomena for these cements.

There is a second stage in these research studies now being conducted at this institute. It consists in checking whether this supposed discontinuity is also reflected in the hardness of the paste, and if so, to establish whether it coincides in time with that observed by electrical procedures. The methods utilized and the results so far obtained (which will be published in detail later) are as follows.

A mechanical device has been adopted,[2] consisting of a toothed wheel which rotates, penetrating about 2 mm below the surface of a cement specimen just molded. The axis of the wheel moves longitudinally all the time, so that the wheel acts continuously on new parts of the paste. The current (intensity) necessary to drive the wheel was measured at close intervals. From this measure it was possible to calculate the work necessary to separate the particles of paste. It has been observed that the work, which was small and almost constant at first, became suddenly much larger at a given stage.

Simultaneous experiments have shown that this change occurs at the same time both along the dielectric constant-time curve and along the *R-t* curve. This fact once again inclines us to regard as reasonable the hypothesis that setting and hardening are consecutive phenomena in the case of portland cement, since the moment at which this change occurs (namely, the increase in force to drive the wheel) corresponds closely to the end of the setting period as indicated by the Vicat needle. Besides, this fact seems to show that the Vicat test is not so arbitrary as we ourselves had imagined (not as regards the nature of the method, but as regards the nature of the phenomenon to which it is applied).

It will be interesting to study (and it is now being attempted, though for the moment no definite data are available) the behavior of high-alumina cements under this dual mechanical and electrical test. The result may perhaps indicate which of the two hypotheses is more acceptable.

For the moment, the study of the depth of penetration of the reaction within the cement granules after a given time shows this to be much greater in the case of high-alumina cement than in portland cements.[3] This indicates the former to have a much greater rate of reaction, which is one of the reasons why high-alumina cements have high initial strength (*cf.*, the penultimate paragraph).

There is, besides, the possibility (indicated by Waters) that there may be materials for which, almost as a limiting case, the *R-t* curves show a continuous increment without a minimum. The intermediate instances observed by Waters, such as plaster of paris, suggest that high-alumina cement might be one of these, with its own peculiarities.

As to the essential difference in the setting of portland and high-alumina cement, as observed by the variation of the electrical resistance of the paste,

the present state of the problem, from the physicochemical standpoint, may be summarized thus:[3]

In spite of the external similarity of the hardening phenomena of portland and high-alumina cement, these are essentially different as regards the chemical reactions which occur in their hydration. The reason is that for portland cement silicates rich in lime predominate, while in high-alumina cement there are more aluminates which are less basic. The hydration reactions of the silicates liberate lime while in the case of the aluminates lime is absorbed and fixed. Hence, in portland cement, which contains silicates and aluminates, both types of reactions (which in a certain sense are opposite) are present. This is not so for high-alumina cement. Although this cement also contains some dicalcium silicate and mono- or dicalcium ferrite, their rate of reaction with water is so slow, compared with that of the aluminates, that they certainly do not participate in the early stages of the hydration process, and only slightly in the latter part of the process.

In contact with water, portland cement immediately forms a strongly oversaturated solution of lime, which slowly attains the saturation concentration. High-alumina cement forms a solution of lime and alumina, whose molar ratio is about 1:1.[4] This varies with the amount of water, but it is hardly sensitive to the greater or smaller amount of alkalies present. Furthermore, the alkalinity of the solution in the presence of portland cement decreases slowly in the course of hydration, while in the case of high-alumina cement this is not so.

During the hydration of high-alumina cement the concentration of alumina in the solution exhibits two maxima, observed by various authors, among them Koyanagi.[5] This indicates the superposition of at least two reactions. This may be assumed to be due to the hydration of the two main calcium aluminates which make up the high-alumina cement, namely, monocalcium aluminate and monocalcium dialuminate. It would be assumed that the hydration of the former develops more rapidly than that of the second. Thus the first maximum would correspond to the solution of the monocalcium aluminate, and the second to that of monocalcium dialuminate.

Although there are authors[6] who admit that both reactions occur separately and consecutively, this cannot be accepted, since the granules of high-alumina cement (in the same manner as portland cement) corrode from outside toward the center. At a given moment during the hydration process both aluminates should begin to react simultaneously with the water. Hence these are two processes which proceed concurrently. The existence of two maxima in the concentration of alumina in the solution is to be explained by the different rate of reaction of each aluminate. These two maxima are not to be attributed to two consecutive processes, but rather to simultaneous phenomena.

Shimizu[7] found differences in the conductivity-time curves for portland and high-alumina cement, in the sense that curves for the latter had a maximum and a steeply decreasing part, while those for portland cement had only the decreasing part.

There are various authors,[8] in addition to those already cited,[4,7] who, by means of calorimetric and conductivity methods, have sought information on the chemical processes associated with the hydration of high-alumina cement.

But it is not only the solution process which is totally different when portland and high-alumina cement are in contact with water. The formation of new solid phases is also different for both. According to Assarsson,[9] hydrated dicalcium aluminate is formed from monocalcium aluminate only at medium temperatures (20 C). At higher temperatures stable hydrated tricalcium aluminate is formed, and below 5 C there is a first stage of unstable hydrated monocalcium aluminate, which tends to become stable.[4] Thus at all stages in the hardening of high-alumina cement one comes against unstable systems, which only attain stability after much time.

As regards hydrated compounds of silica and lime and ferric oxide and lime in high-alumina cement, these are undoubtedly very different from those of portland cement. Considering the hardening process of high-alumina cement, the fact that calcium aluminates are more soluble than the silicates leads one to think that this process must be based on crystallization. However, as a result of microscopic and x-ray investigations, it is now unanimously admitted that the hardening of high-alumina cement is due to colloid chemical transformations.

According to the ideas of Assarsson,[9] the hydration of high-alumina cement has a double character. Firstly there is a solution process in accordance with the scheme of Le Chatelier. Then there is a swelling phenomenon, according to the ideas of Michaelis on hydraulic hardening. This double aspect was emphasized later by Kühl and Berchem.[3]

In addition to that, in the hydration of all hydraulic cements there is a shrinkage, which in the case of high-alumina cement is three times greater than for portland cement.[10] It is not wrong to suppose that this high contraction is one of the essential factors in the rapid initial hardening of high-alumina cement.[3] It must be added that this is a purely physical factor even though it is the consequence of chemical processes.

Finally, we think that there are good reasons to accept as a working hypothesis the fact that the difference between portland and high-alumina cement, in so far as setting is concerned, is due to a partial superposition of the setting and hardening processes in high alumina cement. There are also well established facts which show the profound differences in both the physical and chemical aspects which distinguish high-alumina and portland cements. These may well account for the differences exhibited in the *R-t* curves for these two cements.

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Discussion of a paper by William Prager:

Limit Analysis and Design*

By J. F. BAKER, L. H. N. LEE, and AUTHOR

By J. F. BAKER†

Professor Prager is not quite accurate when he says that "limit design constitutes a practically unexplored field." From his paper it appears that what is called "limit design" in the United States is identical with what we in Great Britain call "plastic design." A considerable team, working during the last 10 years in the Engineering Laboratory, Cambridge University, and before World War II at the University of Bristol, has concentrated its attention on developing a rational design method based on "collapse" of a redundant structure by the development of plastic hinges. Though the team has been responsible for developing many of the methods of analysis now available, some of which are listed by Professor Prager, they have only done this in the course of their interest in design.

Experimental verification, both on small and full scale structures, has gone hand in hand with analysis so as to avoid the usual pitfall, into which applied mathematicians are wont to fall, of choosing simple assumptions because they make pretty analysis possible though they do not represent what really happens in a structure. It has already been possible to make available a simple method which any designer can apply to single story frames[1,2] and many such buildings have been successfully designed and built.[3,4] The design of multistory frames has proved much more difficult because of the complicated behavior of continuous stanchions in the plastic range but the Cambridge team can now design such structures economically. An account of the work will be published shortly.

Even before this post-war investigation at Cambridge, the work was being used widely and at least a quarter of a million tons of steel were fabricated in Great Britain in war-time structures designed according to the plastic method.[5]

Since this work has gone so far in Great Britain, and in view of the close relations which exist between the Cambridge team and the chief American workers, Professor Prager at Brown University, Professor Bruce Johnston of the University of Michigan, and Mr. Beedle at Lehigh University, is it too late to put in a plea for the use in America of the description "plastic

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design" rather than "limit design"? The latter is presumably based on the use of a "limit load" but this, of course, is possible with an elastic method such as that developed in England nearly 20 years ago.[6]

The new and remarkably reliable and simple method is only made possible by plastic behavior; it is surely fitting that this should be reflected in the name.

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By L. H. N. LEE*

Limit analysis of reinforced concrete beams and frames has been carried out both analytically and experimentally in the past decade;[1,2,3,4] but because of the complexity of the behavior of reinforced concrete members, the field remains to be further explored. Professor Prager's timely paper calls attention to the need for useful methods of determining the limiting strength of reinforced concrete structures.

Professor Prager assumes that, for the purpose of limit design, (a) reinforced concrete members cannot bend until the bending moment has reached the yielding moment M' and may then bend indefinitely under the constant bending moment M', and (b) failure of a reinforced concrete structure occurs when sufficient plastic hinges develop to convert the structure into a mechanism.

These assumptions are practically correct or on the safe side for the structures made of ductile materials. But they require further consideration as applied to reinforced concrete structures.

It has been found that the strain capacity of concrete is limited.[2,5,6] The ultimate tensile strain of concrete is about 1×10^{-4} to 2×10^{-4} and the ultimate compressive strain may be 0.3 to 0.5 percent. Because of the limit of the strain capacity, a reinforced concrete member will not bend indefinitely under the yielding moment. The extent of plastic deformation is governed by the properties of reinforcement and concrete, and by the percentage of the reinforcement.[4,7]

For reinforced concrete members with low percentage of reinforcement, the method of limit design can, without doubt, be applied safely. But ordinary reinforced concrete members are designed according to the principle

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of "balanced design." The typical moment-curvature curve of ordinary reinforced concrete members is in the form as described by Professor Prager, except that the plastic range of deformation is limited[4,7] (about 2 to 5 times that of the elastic range). In this case, only under favorable distribution of loads does the yielding of one section in a reinforced concrete structure cause the yielding of other sections. If the yield hinges required to transform a structure into a mechanism have not completely developed, the limiting strength of the structure has to be determined by finding the complete distribution of moment and deformation at the instant of impending collapse.

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AUTHOR'S CLOSURE

The author is indebted to Professors Baker and Lee for their discussion of his paper.

In remarking that true limit design constituted a practically unexplored field, the author wanted to emphasize the fact that at the time his paper was written (November 1952), the overwhelming majority of papers in the field were concerned with limit analysis as distinguished from limit design. The papers mentioned in Professor Baker's references 3, 4, and 6 had not yet been published.

As regards Professor Baker's preference for the term "plastic analysis or design," the author should like to offer the following remark. Limit analysis is the analysis of limiting states of equilibrium and is not restricted to plastic behavior. The theory of earth pressure, the theory of the Voussoir arch,* and the theory of the tension field in aircraft structures constitute other examples of limit analysis. In the last example, the extremely thin web of

*See, for instance, A. Kooharian, "Limit Analysis of Voussoir (Segmental) and Concrete Arches," ACI JOURNAL, Dec. 1952, *Proc.* V. 49, pp. 317-328.

a beam or the skin of a reinforced shell are allowed to buckle elastically. Since the compressive stress normal to the wrinkles is regarded as constant during the buckling process, the general theory of limit analysis can be applied though there is no plastic flow involved. While proper for the application of limit analysis to steel structures, the term "plastic analysis" is not broad enough to include these other applications.

Methods of estimating the elastic-plastic deformation at the time the limit load is reached are certainly highly desirable, as Professor Lee points out. Unfortunately, no reliable methods are known so far, short of the cumbersome study of the way in which the elastic-plastic deformations develop as the loads are gradually increased to the limiting intensity. Moreover, the final deformation depends on the sequence in which the various loads are brought on the structure and increased to their final values. Since, as a rule, this sequence is not known beforehand, the results of the indicated elastic-plastic analysis are of doubtful value unless all possible loading processes are considered to find the one that causes the most severe deformations.

Discussion of a paper by E. W. Scripture, Jr., S. W. Benedict, and D. E. Bryant:

Floor Aggregates*

By JAMES E. BACKSTROM, C. F. MOHR, and AUTHORS

By JAMES E. BACKSTROM†

In general, results of tests performed on nonmetallic aggregates in the laboratory of the Bureau of Reclamation on the abrasion resistance of concrete agree with those reported by Messrs. Scripture, Benedict, and Bryant. This is true even though methods of test and abrasive mediums were different from those used by the authors. In the Bureau tests, it was found that the shot-blast,[1] steel ball,[2] and high velocity water jet (cavitation)[3] methods gave losses in the same order, the only differences being in magnitude.

Witte and Backstrom of the Bureau[1] reporting on an extensive series of shot-blast abrasion tests involving concretes containing six different percentages of air for each of 11 water-cement ratios (0.43 to 0.70) concluded, as did the authors of the paper under discussion, that for a given aggregate, "compressive strength is the most important factor controlling the abrasion resistance of concrete; abrasion resistance increasing as compressive strength increases." They also reported that small changes in compressive strength were reflected in the results of the shot-blast tests for concretes having compressive strengths of less than 4000 psi but that the test was not sensitive to small changes for concretes having strengths in excess of 4000 psi. These data show that if the abrasion resistance developed by 810-psi concrete is assumed as unity, then relative abrasion factors of 2, 4, 8, and 16 are obtained with the same aggregate at strengths of 1600, 3200, 5700, and 7600 psi, respectively.

Results from a recent series of steel-ball abrasion tests including ten natural (water-worn) and crushed aggregates and combinations of these in floor topping concretes having water-cement ratios of 0.32 and 0.36 indicate that for a particular combination of fine and coarse aggregate, abrasion resistance varies directly with compressive strength. Two relatively soft limestones used in this series developed uniformly poor resistance to abrasion even though one of the limestones produced the highest strengths of the group tested. The abrasion resistances of concretes made with comparatively hard natural and crushed basalt and granite aggregates were relatively high with only slight differences shown in the results although the strengths developed

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varied from 6700 to 11,300 psi. Apparently no correlation exists between the abrasion resistance of high-strength concretes containing all hard or all soft aggregates and concretes made with a mixture of these aggregates. A mixture of soft fine aggregate and hard coarse aggregate, or a mixture of soft coarse aggregate and hard fine aggregate may produce concrete of comparatively high or low abrasion resistance.

Investigations are continuing using the same ten aggregates mentioned above in weaker concretes to evaluate the effect of aggregate quality on abrasion resistance of average strength (3000 to 5000) concretes. Results now available show that in the range of 3000- to 5000-psi concrete the hardness of coarse aggregate significantly influences the abrasion resistance.

Other limited data indicate that for a given good quality aggregate as the percentage of coarse aggregate is increased the abrasion resistance is increased.

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By C. F. MOHR*

The writer would like to take exception to some of the test results reported which indicate that trap rock does not resist wear better than softer aggregates. In their conclusions the authors state that there is no relation between the hardness of the aggregate and resistance to abrasion, and that malleable iron aggregates show about 400 percent greater resistance to abrasion than the brittle aggregates in which trap rock is included. This is contrary to field experience reported in our files.

It is believed that results obtained from the testing machine used by the authors are erroneous because it does not measure impact, only abrasion under a light uniform rubbing motion, which seldom occurs under actual service conditions where there is a combination of impact and abrasion. Such wear can be measured effectively by the Los Angeles rattler test—where trap rocks, the basic igneous rocks such as basalt, diabase, and gabbro, show much lower wear than slag, marble, limestone, granite, and silica aggregates.†

The writer agrees with the authors' conclusion that abrasion resistance increases with the increase in crushing strength of concrete.

To produce a heavy-duty, wear resistant concrete floor, many contractors in this area use a no-slump concrete, vibrated and machine finished, using a trap rock aggregate with a high crushing strength and no admixture. The

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†"Results of Physical Tests of Road Building Aggregates," U. S. Bureau of Public Roads, 1953, p. 10.

hard tough aggregate functions to produce a high crushing strength concrete. It is, of course, necessary that first class workmanship, mixing, finishing, and curing be used.

A typical trap rock, from around Lambertville, N. J., listed as diabase or gabbro, has shown up well in tests. *Research Paper* No. 1320, National Bureau of Standards, Aug. 1950, lists test results on 116 samples of granites including trap rock or black granite, listed as gabbros and basalts, from various parts of the United States. The Lambertville gabbro (pp. 176 and 185 of the report) showed the highest average crushing strength, wet and dry, of 43,350 lb on 2-in. cylinders—more than twice that of most granites.

Such results are confirmed by actual service performance, a few typical examples of which are mentioned below.

In the Philadelphia metropolitan area, where the earliest portland cements were produced in the United States, some of the first concrete sidewalks were laid on a large scale in the central city section. Many of these old sidewalks are 40 and more years old and still in service. The only aggregate produced on a large scale at that time was trap rock which was used exclusively for railroad track ballast (and is still used in this area for heavy-duty track ballast). The smaller sizes of either basalt, diabase, or gabbro were screened out for concrete sidewalks. In the area surrounding City Hall in central Philadelphia, the old part of the city, these sidewalks are easily identified because it is the only dark aggregate used and of irregular shape.

The following is a typical report. Chestnut Street (north side between Broad and 13th Street) is said to carry the heaviest concentration of foot traffic in the city. The section alongside the Weidner Building was laid in 1914 using trap rock. The adjoining sidewalk using Delaware River gravel was laid in 1932. Both sections were done in first class workmanship. An inspection shows the trap rock section worn smooth and even because all particles are of uniform hardness. The gravel surface, with 18 years less wear, shows much wear in the center section where most walking occurs. It is rough and uneven, the hard quartz particles extending above softer sandstone and other types of rock in the gravel. Such an uneven surface would create rapid wear due to impact of moving wheel loads in an industrial floor.

With few exceptions no trap rock sidewalks were laid after about 1925 when gravel, slag, and limestone replaced the higher priced trap rock. All these softer aggregate sidewalks show much more wear than the harder trap rock sidewalks. The trap rocks were diabase, gabbro, and basalt with a Los Angeles rattler loss of 13–16 percent, limestones and slags with 35–50 percent Los Angeles rattler loss, and gravel about 25–35 percent loss according to Pennsylvania State Highway Department tests.

Should one wish to compare wear of concrete surfaces subjected to wheel loads instead of foot traffic, two examples follow. In Upper Darby of suburban Philadelphia, 69th Street from West Chester Pike to Chestnut Street was a job mix laid in 1927. The contractor had truck loads of trap rock and limestone delivered to the mixer and placed at random. The limestone had

about twice the Los Angeles rattler loss of the trap rock. Using a straight edge, the limestone spots of the pavement are worn 1 to 2 in. lower than the trap rock sections. Another example is Route 1, Baltimore Pike, beginning at Cobbs Creek City and going south into Delaware County. The west side of the pavement was Morrisville-Delaware River gravel; the east side was a diabase trap rock with about one-half the Los Angeles rattler loss. Both sections were placed about 25 years ago by the same contractor. The gravel concrete has worn rough and uneven 2 to 3 in. more than the trap rock sections.

The performance of aggregate under these conditions is also applicable to heavy-duty floors, where it is important that the surface have a high resistance to wear and that the surface will wear smooth and even. It is the coarse aggregate that determines the rate of wear.

AUTHORS' CLOSURE

The discussions by Messrs. Backstrom and Mohr are appreciated.

It is gratifying to learn that the results of tests on nonmetallic aggregates conducted by the Bureau of Reclamation agree with those of the authors, particularly in view of the fact that the methods of conducting the abrasion tests were different.

With reference to Mr. Mohr's belief that the results are erroneous because the testing machine used does not measure impact, the abrasion machine was essentially identical to the machine developed by the National Bureau of Standards for the evaluation of the abrasion resistance of floors as described in the paper and in reference 4 thereto. The results obtained by the authors with metallic aggregates are in accord with the National Bureau of Standards data. Mr. Backstrom shows in his discussion that this type of abrasion (his steel ball apparatus) correlates well with other types such as shot-blast and high velocity water jet (cavitation), which do involve impact.

Mr. Mohr, early in his discussion, states that certain conclusions of the authors are contrary to field experience reported in his files. It is not clear, however, whether he refers to the conclusion that there is no relation between the hardness of the aggregate and resistance to abrasion, or the conclusion that malleable iron aggregates show about 400 percent greater resistance to abrasion than brittle aggregates, or both. He cites no data or field experience having to do with iron aggregates. It seems obvious that the particular property of iron aggregate which causes it to resist abrasion or impact is malleability. It is suggested that aggregates without this property will resist abrasion to an extent which presumably is some function of their hardness, or perhaps more accurately their brittleness, but once this resistance is exceeded, particularly with respect to impact, their resistance immediately drops to essentially zero, *i.e.*, they become dust. The same supposition would apply to the mortar. Malleable iron, on the other hand, to a degree far greater than a nonmalleable material, tends to flatten under impact or smear under abrasion and, in some cases at least, thereby extends over and protects

the mortar. This supposition should not, of course, be carried to the point where any lessening of the quality of the mortar should be tolerated.

The authors' paper reports results on relatively high strength mixes, 4000 psi for the monolithic slabs and 11,500 psi for the topping mixes and dust coats, such as would be used for high quality floors. That somewhat different relations might exist, as is suggested by Backstrom, for low strength mixes seems quite probable. With a low strength mortar the harder aggregates might well afford some protection to the mortar under certain types of wear.

With respect to the service records cited by Mr. Mohr, while the "proof of the pudding is in the eating," it is practically impossible to draw any firm conclusions from miscellaneous field installations. The conditions from one installation to another are seldom comparable. Sidewalk and highway pavements are normally quite different from floors and floor toppings, both with respect to the type of wear or abrasion to which they are subjected and particularly with respect to exposure to weathering. Floors are seldom subjected to weathering, whereas the field installations in Pennsylvania cited by Mr. Mohr almost certainly were exposed to rather severe weathering during the winter seasons. It is suggested that the good and poor conditions of the pavements are a reflection of their ability to resist weathering rather than to resist abrasion.

If a pavement has been damaged by frost action it, of course, will have poorer abrasion resistance. There is no doubt that a good pavement can be made with trap rock or almost any other aggregate of reasonable quality. It is also possible to lay a poor pavement with trap rock or any other aggregate.

The type of abrasion which a concrete surface must resist varies greatly with the service conditions to which it is subjected. The design of the concrete or topping should be based on the particular conditions. Pavements such as highways, which are primarily subjected to rubber tire traffic; sidewalks subjected primarily to foot traffic; spillways or conduits subjected to the scouring action of sand and gravel and sometimes to cavitation; and industrial floors subjected to the action of steel tires and the dropping of heavy objects, should be designed quite differently for best performance. The paper under discussion primarily was concerned with the latter condition and it was with this particular condition in mind that the tests were made and the conclusions drawn.

Results of the investigations now in progress by Mr. Backstrom are awaited with interest. The authors would be greatly interested also in any specific data in the possession of Mr. Mohr with respect to the relationship between the hardness of aggregates and resistance to abrasion, or comparable data on the resistance to abrasion of malleable iron aggregates and brittle aggregates in connection with their use in floors.

Discussion of a paper by Grover L. Rogers:

Validity of Certain Assumptions in the Mechanics of Prestressed Concrete*

By Y. GUYON, FRANCO LEVI, G. MAGNEL, and AUTHOR

By Y. GUYON†

I do not doubt the results obtained by Professor Rogers, but I think that general conclusions on the whole problem of two-way prestressed slabs should not be made from this test. It is a particular case (square slab) with bearing conditions which are not generally encountered in construction (simply supported along the edges). In this particular case, Professor Rogers found no redistribution of moments before cracking.

A number of conditions are necessary so that a redistribution of moments may be possible:

1. It is necessary that the load can be carried by successive "carrying systems," one system assuming further loads when the other system has reached its resisting limit.

For instance, if we consider a long rectangular slab, supported on monolithic beams on four sides, we will have at least four possible carrying systems which may be schematically represented in the following way: (a) Bending of strips parallel to the short sides, following the elastic theory of slabs, with fixed-end moments deduced from the same theory. (In this scheme the help afforded by strips parallel to the long side is neglected.) (b) The zone under load becoming plastic (or having reached its tensile resistance) can no longer transmit any further moment. Since the neighboring strips have not reached their resisting limit, the additional load can be transferred to them by shear. (c) The loaded strip becomes, because of plasticity under the load, equivalent to two cantilevers monolithic with the long beams; the fixed-end moment can thus increase, the central moment remaining constant. (d) The load can be carried by a "pressure dome" which appears inside the slab and which finds its abutments at the corners of the slab where the beams cross.

2. It is necessary that the successive carrying systems have different resistances, or more exactly, different "reserves" of resistance after the end of the elastic phase. For instance, in the case of a circular slab, simply supported along its edge and loaded at its center, there are two successive carry-

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ing systems: the full slab at the beginning; and next in effect, the slab with a central hole assimilating the plastic zone, which can no longer transmit further moment. The second system cannot extend the life of the slab, because the distribution of moments is not altered along the perimeter of the hole.

3. If cracking is concerned, a third condition is necessary, *i.e.*, that the material be ductile.

The fact that Professor Rogers' test has not shown increasing of resistance before cracking, in respect to elastic theories, seems to me to demonstrate, not that the elastic theory is applicable in all cases, but that the necessary conditions were not fulfilled in the particular case. One could assume that the third condition above (concrete ductility) was not fulfilled, and that moment redistribution which is indubitable before rupture in all statically indeterminate structures, was not possible before cracking.

Recent tests which I have performed, and where I have tried to fulfill the first two conditions, have led to results which I consider as proof of redistribution before cracking in the case of slabs.* Six rectangular continuous slabs, 1.25 x 3 m (4 x 10 ft), 8 cm thick (about 3 in.), and prestressed in two directions at 14 kg per sq cm (200 psi), were successively loaded in the center up to the first crack.

The usual elastic theories would have authorized a load of 2700 kg (6 kips) before cracking, taking into account the tensile resistance. The actual cracking loads have been 4000 to 4800 kg (9 to 10.5 kips) for the various slabs. One could object that the elastic theories which we had used were not correct because they were unable to take into account the real bearing conditions, and, in fact, generally empiric formulas are used to evaluate the fixed-end moments. However, the diagrams from strain gage readings show modification regarding linear laws before cracking. These results will be discussed in a forthcoming paper.

I think—and Professor Rogers' test does not contradict this assumption—that the total tensile resistance of concrete can be taken into account for the slabs. There is, in fact, an essential difference between a slab and a beam when cracking is concerned. In a beam, if the stress meets a zone of least resistance, a crack necessarily occurs and there is rupture of contact. In a slab there is no such rupture of contact, because the stress finds ways on the right or on the left. Therefore, while in a beam the resistance to be considered is the *minimum* resistance, in a slab it is the *average* resistance. A reasonable coefficient of safety may be taken on this average resistance, but it would be unreasonable to rule, as do many specifications, that this tensile resistance must be ignored.

By FRANCO LEVI†

The results obtained by Professor Rogers are interesting and certainly

*A distinction between slabs and beams is made for reasons which will be clearer after publication of the paper reporting the test results.

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constitute a valid contribution to the study of the behavior of highly indeterminate prestressed structures.

The writer can but share Professor Rogers' opinion about the interpretation of the apparent divergence between his results and Mr. Freyssinet's. There is indeed no doubt that the cracking load considered by Mr. Freyssinet in the experiment at Orly cannot be considered as the "first cracking load." For values of load much less than the maximum reached, other cracks had to appear on the invisible bottom face of the slab. The load reached by Mr. Freyssinet must therefore be considered as next to the limit of resistance of the slab and not as the initial cracking load.

That this ultimate load is much greater than the one foreseen by the elastic theory is not surprising. On this value of the load certainly acts the redistribution effect of stresses given by the first cracks appearing on the bottom face in a zone adjacent to the load. This last affirmation is justified by the results obtained by the writer in an experimental slab.*

Attention must also be fixed on another aspect of the results obtained by Professor Rogers: the almost perfect coincidence between the experimental value of the cracking load and the forecasts founded on the elastic theory. Here we must remember that in the field of prestressed beams Mr. Guyon has demonstrated that the origin of plastic strains in the extended zone delays the appearance of the first cracks and even more so as the value of prestress increases. It is surprising that on his slab Professor Rogers has not revealed an analogous effect, especially since the value of prestressing was rather high in this particular case.

This observation assumes special significance considering that on the slab the local effect of the plastic strains is certainly accompanied by an hyperstatic effect of redistribution among the various strips making up the slab.†

A possible explanation for the disappearance of the above mentioned plastic effect might be that Professor Rogers had overvalued the tensile strength of the concrete. We also think that the slab may have had a preestablished capillary crack in the zone in which the first visible cracks appeared. It would be interesting if the author would give the reasons which, from his point of view, justify this apparent contradiction.

By G. MAGNEL‡

I have not read Professor Rogers' paper after its publication as I followed the test as it was carried out in this laboratory. I would simply like to say how much I have appreciated the ability of the author in both theoretical calculations and in experimental work.

I would like to add that many other experiments made in this laboratory show that the cracking load of a prestressed structure can be determined by using the elastic theory, with an accuracy of 10 percent for statically deter-

*Levi, F., "Theoretic Experimental Investigation Beyond the Elastic Limit of a Prestressed Slab on an Elastic Foundation," (in French), *Annales de l'Institut Technique du Batiment et des Travaux Publics* (Paris), V. 6, June 1953, Series: Beton Précontraint, No. 16, pp. 535-556.

†Levi, F., "Superfici d'Influenza e Fenomeni di Adattamento Nelle Lastre Piane," *Giornale del Genio Civile* (Rome), V. 90, No. 5, May 1950.

‡Professor and consulting engineer, Department of Civil Engineering, University of Ghent, Ghent, Belgium.

minate as well as for statically indeterminate structures.

This has been confirmed recently by tests carried out in this laboratory by Professor T. Y. Lin of the University of California. Plastic adaptation in statically indeterminate structures only becomes an important factor for loads higher than the cracking load.

AUTHOR'S CLOSURE

The author is pleased to acknowledge the comments which have been submitted by Messrs. Magnel, Levi, and Guyon. The discussions of these internationally eminent engineers in the field of prestressed concrete have greatly enhanced the value of the original paper.

There are three phases to be considered in calculating the response of concrete structures to static loads. First, we have the essentially elastic stage where there is a complete reversibility of stresses and deformations. This phase has been studied extensively. Second, we have the post-elastic phase which terminates with the appearance of the first tensile crack. This is the phase under scrutiny in this paper. Third we have the post-cracking phase which terminates with ultimate collapse of the structure. This is the phase studied by investigators interested in limit analysis and ultimate load design. The tests at Orly, for instance, relate to this third phase.

The question considered here is concerned with the length of the second phase and especially whether elasticity theory can be used for predicting the load necessary to cause the first crack to occur. Professor Magnel indicates in his discussion that elasticity theory is adequate. The author's test results support his contention.

In theory, however, we may conclude otherwise since we can show that the nature of the internal constraints offered by the structure may be such as to keep large deformations (and subsequent cracks) from occurring even though the elastic limit is exceeded.

Mr. Guyon rightly points out that a square slab may not offer such constraints but it must be remembered that the nature of the load as well as the geometry of the slab will determine the probability of such internal constraints existing. In choosing the loading for his test, the author was careful to choose the double loads so as to be sure of providing a secondary "carrying system" and thus to more accurately test the hypothesis.

Regarding the nature of the supports used for slab tests, the author feels that as long as the support conditions cannot be accurately considered in our analysis one can never be certain as to their effects on the test results. It was this reason which prompted the use of the simply-supported slab in his test. He looks forward, however, to the opportunity of studying Mr. Guyon's most recent test results.

Professor Levi has wisely pointed out the possibility of an overevaluation of the concrete strength used in the test or the possible occurrence of a capillary crack. The author feels that the extent of error due to these factors must be very small.

Discussion of a paper by James S. Blackman:

Method for Estimating Water Content of Concrete at the Time of Hardening*

By DELMAR L. BLOEM and AUTHOR

By DELMAR L. BLOEM†

The accuracy of the method described by the author depends upon the validity of two assumptions: (1) that 24-hr immersion brings the weight of the concrete to its original value, and (2) that the weight after drying is equal to the original weight of the dry solid ingredients. The author's data indicate that these assumptions are valid for the limited range of variables which he studied—*i.e.*, water-cement ratios for the major portion of the work covering a range of only 0.455 to 0.539; all concrete non-air-entrained; and all specimens laboratory moist cured for 28 days. This writer has reason to doubt that the first assumption (that 24-hr immersion restores the sample to its original weight) is valid for the broader range in classes of concrete and curing conditions encountered in the field.

Tests made in the Joint Research Laboratory sponsored by the National Sand and Gravel Assn. and the National Ready Mixed Concrete Assn. at the University of Maryland indicate that the weight of a concrete specimen after 24-hr immersion may vary greatly in relation to its original weight, depending upon the characteristics of the concrete and the conditions under which it was cured. Eight types of concrete were studied—air-entrained and non-air-entrained with cement factors of 4 and 7 sacks per cu yd and slumps of about 2 and 7 in. Cylinders, 4½ x 9 in., were molded with each type of concrete and cured under four conditions, as follows, before being immersed in water for 24 hr: (1) air-dried 27 days; (2) standard moist-cured 6 days; (3) standard moist-cured 6 days, air-dried 21 days; and (4) standard moist-cured 27 days. All specimens remained in the molds surrounded by wet burlap for the first day after molding before being stripped and cured as indicated.

Table A shows the amount by which the weight of the specimens after immersion for 24 hr differed from their weight at 1 day. Other tests have shown that the 1-day weight is essentially the same as the weight of the fresh concrete. The weights have been converted to gallons per cubic yard, the unit customarily used for mixing water. It will be observed that the

*ACI Journal, Mar. 1954, *Proc.* V. 50, p. 533. Disc. 50-31 is a part of copyrighted Journal of the American Concrete Institute, V. 26, No. 4, Dec. 1954, Part 2, *Proceedings* V. 50.

†Assistant Director of Engineering, National Sand and Gravel Assn., Washington, D. C.

TABLE A—EFFECT OF COMPOSITION AND CURING ON MOISTURE CONTENT OF CONCRETE*

Cement, sacks per cu yd	Water-cement ratio		Slump, in.	Percent air	Gain in water after 1 day, gal. per cu yd, for various curing conditions†			
	Gal. per sack	By weight			0 M.R. 27 Air	6 M.R. 0 Air	6 M.R. 21 Air	27 M.R. 0 Air
Non-air-entrained concrete								
4	9.12	0.81	1½	1.7	2.2	3.6	0.2	4.1
4	10.10	0.90	6½	0.4	2.9	5.6	2.1	6.3
7	4.75	0.42	1½	1.1	2.7	4.0	2.7	4.8
7	5.53	0.49	7	0.0	3.9	6.0	4.2	6.2
Air-entrained concrete								
4	7.47	0.66	2	8.1	2.5	3.2	0.9	4.7
4	9.14	0.81	7	6.6	2.4	4.1	1.9	5.9
7	4.49	0.40	2½	5.2	2.1	3.3	2.1	4.4
7	5.18	0.46	6½	5.3	3.2	5.0	3.4	6.9

*Specimens 4½ x 9-in. cylinders, cured as indicated and then immersed in water for 24 hr. Values represent net gain in weight from age of 1 day to end of 24-hr immersion, expressed in gallons per cubic yard of concrete.
Note: Auxiliary tests showed that specimens lost essentially no weight from time of molding to weighing at 1 day.
†Headings indicate days in standard moist room at 73 F and days in 70 F air at constant humidity of about 60 percent after 1 day in molds completely surrounded by saturated burlap.

weight of the concrete after 24-hr immersion in all cases exceeded the original weight and that the amount of the excess ranged from the equivalent of 0.2 to 6.9 gal. of water per cu yd.

Curing condition made the greatest difference in amount by which soaking affected the weight of the concrete. The water content of specimens moist-cured 27 days exceeded by as much as 4 gal. per cu yd that of specimens moist-cured 6 days and air-dried 21 days. Composition of the concrete had a less marked, but still significant, effect. The higher slump concretes contained from about 1 to 2 gal. per cu yd more water after soaking than the drier mixes. Except for the lean, dry mixes, the air-entrained concretes generally had water contents slightly lower than those without entrained air. Also, in general, the rich concretes tended to have a higher indicated water content than the lean mixes, although this tendency was not consistent for the specimens which received continuous moist curing for 28 days.

It appears that the drying test described by the author may give a rough indication of mixing water content but, due to differences in the extent to which soaking will restore the weight of the concrete, it is far from exact. Nevertheless, tests of this kind merit further study since there is need for methods to evaluate the quality of hardened concrete and diagnose failures. Some additional possibilities along this line are demonstrated in Fig. A and B. These show data from the tests referred to earlier which formed the basis for Table A.

Fig. A shows the relationship of water-cement ratio to 24-hr absorption for the eight classes of concrete and two different curing conditions. One set of specimens was cured in air at 70 F after 1 day in molds; the other set was cured in the standard moist room at 73 F to the age of 7 days and thereafter in air at 70 F. The absorption tests were made at the age of 28 days.

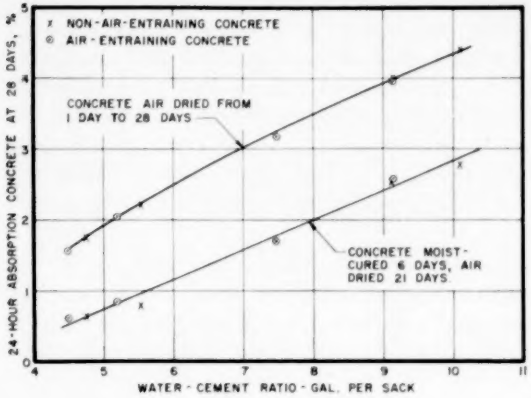
x NON-AIR-ENTRAINING CONCRETE
◎ AIR-ENTRAINING CONCRETE
CONCRETE AIR DRIED FROM 1 DAY TO 28 DAYS
CONCRETE MOIST-CURED 6 DAYS, AIR DRIED 21 DAYS
24-HOUR ABSORPTION CONCRETE AT 28 DAYS, %
WATER-CEMENT RATIO-GAL. PER SACK

Fig. A—Relationship between water-cement ratio and 24-hr absorption of concrete (Series 125)

The relationship between absorption and water-cement ratio is excellent for either curing condition with absorption increasing as water-cement ratio increased, independent of whether or not the concrete was air-entrained. It is obvious, however, that such an absorption test would be of little value as a means of determining water-cement ratio unless there were knowledge of the adequacy of curing. It is not inconceivable that this deficiency could be overcome by giving concrete samples a period of supplemental curing and drying in the laboratory prior to the absorption test.

Fig. B demonstrates the possibility of using absorption tests as a means of determining the original air content of samples of hardened concrete. Abscissas are air contents of the fresh concrete, measured gravimetrically, and ordinates are absorptions of oven-dried specimens soaked for 24 hr or saturated under vacuum. The latter specimens were subjected to vacuum (maximum residual pressure 1 cm of mercury) for 1 hr, after which the vacuum was broken with water and the specimens allowed to soak for 24 hr. In these tests, the cylinders were oven-dried to essentially constant weight at 225 ± 2 F before being tested for absorption. Each point represents the average

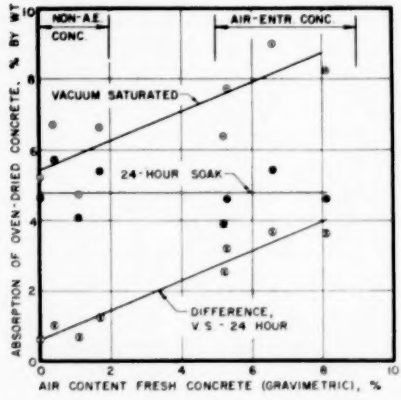

Fig. B—Relationship of absorption to air content of concrete (Series 125)

for the four cylinders originally used in the study of different curing conditions.

It appears that the voids resulting from air entrainment are not readily filled during immersion in water for 24 hr but do become filled to some extent under vacuum saturation. Observe that, while there was no tendency for 24-hr absorption to increase with air content, there was a marked increase in the absorption after vacuum saturation. The difference between absorption after 24-hr immersion and after vacuum saturation appeared to be closely related to the initial air content.

Although the data discussed above are too limited in scope to provide generally applicable relationships, they do suggest that absorption and moisture content characteristics might be useful for diagnosing the quality of hardened concrete, particularly as to water content, water-cement ratio, and air content.

AUTHOR'S CLOSURE

The author appreciates the interest of Mr. Bloem, and agrees that a wider spread of water-cement ratios and types of curing would have been desirable. However, the data from the limited investigation were published in the hope that some hitherto unpublished data bearing on the problem would be brought to light. As it turns out that hope has been realized.

The author agrees with Mr. Bloem that complete saturation of hardened concrete cannot be expected after 24-hr immersion at normal temperatures for the broader range in classes of concrete. It is easily possible that immersion under a vacuum could improve the completeness of saturation. However, it might also create an additional error by producing a heavier saturation in the more absorptive coarse aggregates.

The additional data on amounts of water absorbed by concrete cured under different conditions are a helpful addition to the ideas presented in the method.

Discussion of a paper by Jack R. Janney:

Nature of Bond in Pre-Tensioned Prestressed Concrete*

By P. W. ABELES, K. HAJNAL-KÓNYI, N. W. HANSON, and AUTHOR

By P. W. ABELES†

Mr. Janney's paper contains interesting test results on the bond of pre-tensioned wire in prestressed concrete, and his illustrations of the gradual movement of the maximum bond stress at increased loading are instructive. The writer would like to deal with this question more from a practical point of view and, in this connection, later discusses some particulars of Mr. Janney's tests.

When a prestressing steel is to be used for pre-tensioning it is essential that no appreciable slip takes place under any loading. Consequently, any steel the surface conditions of which are unsatisfactory for the quality of concrete used, so that slip or bond failure takes place, is unsuitable for pre-tensioning. Bond failure or slip may occur also due to too close spacing of the wires, which ought therefore to be avoided. It is not the transfer of the prestress but ultimate load conditions which govern the suitability of the steel.

The writer remembers the first test with which he was associated on prestressed concrete beams containing pre-tensioned wires. These tests were made in London in 1941, and the wire slipped at cracking. In this instance, the lubricant on the surface of the wires had been removed only by wiping with a rag. Consequently, in later tests described in the ACI JOURNAL in 1945[1] the wires were first pickled to remove any lubricant, and this was satisfactory.

In Great Britain where 0.2-in. diameter wire has been employed in pre-tensioning work for about 10 years, it was found necessary to clean the wire thoroughly with a carborundum wheel until wire manufacturers were prepared to clean the wire at the plant and provide by special treatment satisfactory surface conditions which ensure satisfactory bond.

It is interesting to note that piano wire was originally considered the only suitable material for pre-tensioning by Hoyer, whose long line system is limited to the use of wire of 2-mm diameter; such piano wire is relatively much smoother than patented wire of 0.276-in. diameter. Also in investigations in Switzerland in 1946,[2] it was stated that wire of greater diameter

*ACI JOURNAL, May 1954, *Proc.* V. 50, p. 717. Disc. 50-44 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 26, No. 4, Dec. 1954, Part 2, *Proceedings* V. 50.

†Consulting Engineer, London, England.

than 0.12 in. (3 mm) was unsuitable and could only be used when special indentations were provided. However, fatigue tests on prestressed concrete members containing British patented smooth 0.2-in. diameter wire have shown that satisfactory bond is ensured under repeated loading, even in the range in which cracks opened and closed millions of times during the test.[3] Similar tests are now being carried out by the research department of British Railways on beams of 13-ft 6-in. span containing pre-tensioned smooth 0.276-in. diameter wire. Results so far have been very satisfactory.*

Mr. Janney has quite rightly pointed out that the pre-tensioned wire has much greater bond resistance than an untensioned wire, since a greater friction and side pressure is obtained at the ends due to swelling of the wire at release, and further contraction of the tensioned wire under ultimate load conditions is less than that of untensioned wire. Consequently the severest tests for the suitability of wire is a failure test on a beam containing untensioned wires. If slip does not occur, and the entire strength of the steel is reached at failure, the wire is certainly suitable as prestressing steel. However, this test may be too severe, since for instance 0.276-in. diameter smooth wire has proved to be entirely satisfactory for pre-tensioning for concrete of definite strength, but is not fully satisfactory in an untensioned state as a single wire, though it might be satisfactory when used as twin twisted wires. In addition to surface conditions concrete strength and spacing of the wire are important. Surface conditions can be greatly improved by providing indentation, or better bond may be obtained by the use of wires twisted together. Strands of ½-in. diameter were satisfactorily employed for pre-tensioning in some recent tests in Denver, Colo.

Eq. (9) of the paper, which is certainly of great advantage in analyzing the bond stress in tests due to the increase in horizontal shear, should, in the writer's view, not be used for design purposes because of variation of the appropriate bond stress. The designer requires the following data: (1) Are the surface conditions of a specific prestressing wire satisfactory to avoid slip and bond failure? (2) What is the required minimum concrete strength to ensure this? (3) What is the minimum permissible horizontal and vertical spacing? (4) What is the transfer length and which of the curves shown in Fig. 3 applies? This would have to be ascertained for each type of wire and one could then dispense with formulas for design purposes.

The writer would like to refer to R. H. Evans' investigations[4] in which the distributions of the strains at the ends of pre-tensioned wires of different diameter were investigated, theoretical formulas derived based on the thick cylinder analogy, and an approximation shown from the thick cylinder theory. In these formulas by Professor Evans the friction value and Poisson's ratio were also taken into account. In his tests he used wires of different diameter and investigated them at different ages. It was found that due to creep, a

*These tests have now been completed. One beam was subjected to nearly 10 million repetitions the upper limit of which was gradually increased from ½ to 2/3 of the maximum static failure load. The lower limit was ¼ of this load. At the 10th million repetition, one wire failed in fatigue and the beam was still capable of withstanding over 30,000 load repetitions with an increased upper limit of approximately ⅔ of the static failure load.

considerable increase in transmission length occurs after a time; this was established by comparative tests after 1 day, 1 month, 6 months, 1 year, and 2½ years. Such formulas as shown by Professor Evans and Mr. Janney are valuable for comparing various test results, but should not be used for design purposes, since they contain too many unknown variables. Practical tests on specimens using a special type of wire and concrete of specific minimum strength and compaction are essential.

Reduction of the transmission length to a minimum is important only in special instances, where large bending moments and/or shear forces occur near the ends, as with railroad ties or short cantilevers. In such cases it is necessary to select a suitable type of prestressing steel ensuring special mechanical bond. For simply supported and uniformly loaded beams, however, the transmission length will be of little influence since the bending moments increase less rapidly as the prestress is developed. In most cases it does not matter whether the prestress is fully transferred in 6 in. or in 30 in.; normally there is sufficient prestress available quite near the ends to ensure satisfactory principal stresses. It may be mentioned that a transmission length of 2 ft 6 in. or even more is of no harm in simply supported, uniformly loaded beams of a span of more than 14 ft, as tests have proved.

Also of great importance is the spacing of the tensioned wires. The writer made some tests on the spacing of 0.2-in. diameter wires and found that a distance of ¾ in. horizontally and ⅜ in. vertically appears satisfactory for concrete of cylinder strength of approximately 5000 to 5500 psi. A smaller distance (for instance ¾ in. horizontally as before and 0.2 in. vertically, that is, all wires touching vertically) leads to failure at a load corresponding to approximately 90 percent of the maximum possible if the tensile strength of the steel is fully used. Previously it was known that spacing of pairs of straight tensioned wires at a distance of 1 in. horizontally and vertically was satisfactory, since the strength of the steel was fully used.

From Mr. Janney's testing arrangement it was to be expected that a crack would develop under the center load, where electric strain gages were fixed to the wires. However, a single point load has the disadvantage that the exact magnitude of the bending moment is uncertain in view of the unknown load distribution, though it may be likely that no such distribution occurs in the event of a wide crack under a point load. It is a pity that this is not quite certain, since the failure load of beam 5-r results, according to Table 3, in an ultimate steel stress of 206,000 psi in a nominal lever arm jd of 9.2 in. Depth of the reinforcement is 8.75 in. and the ultimate tensile force for the ultimate steel stress amounts to 17,000 lb, which may be balanced by a rectangular stress block in the concrete compressive zone of 0.63 in. depth for a stress of 4500 psi (f_c'). Consequently, the maximum possible actual lever arm is 8.44 in. and the ratio between nominal and actual depth is 9.2/8.44 = 1.09. It would have been interesting to know whether this excess in stress was really reached. Such an excess over the maximum stress of the steel within a crack of limited width appears quite plausible because of the

reduced necking over a limited length. In this connection it may be pointed out that so far nobody has been able to measure the exact strain conditions in and adjacent to a crack in concrete with well bonded steel. This is certainly an important problem which would deserve special research. To be able to analyze Mr. Janney's test results also for ultimate load conditions, it would be helpful if complete stress-strain curves of the various prestressing steels were shown in the closure.

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By K. HAJNAL-KÓNYI*

Mr. Janney's paper is a valuable contribution to the question of bond between high strength wires and concrete. It shows clearly the importance of the surface conditions of wires in pre-tensioned concrete members the safety of which depends entirely on bond.

A few years ago the writer carried out tests on beams reinforced with untensioned high strength wires of 0.104-in. diameter[1]. The percentage of the reinforcement was 0.18 percent, *i.e.*, within the range covered by Mr. Janney's beams. Prior to the tests described in reference 1 a similar beam failed by bond soon after the first cracks had occurred because the wires were not sufficiently cleaned before they were embedded in concrete. A description of this beam was outside the scope of the writer's paper but in connection with Mr. Janney's research it may be of interest to recall that the calculated average bond stress at failure was approximately 100 psi, in close agreement with Mr. Janney's beam No. 1-c.

It should be noted that the percentage of the reinforcement in Mr. Janney's beams did not exceed 0.2 percent which is very low and, except for composite structures, is not representative of practical applications. Tests carried out by Revesz[2] have proved that in concrete of a cube strength of 7650 to 7840 psi, prestressed wires of 0.105-in. diameter of a tensile strength of approximately 300,000 psi can be fractured even if the percentage is as high as 0.77 percent provided the bond is satisfactory.

There is one statement in Mr. Janney's paper which needs further clarification. In commenting on Table 3 Mr. Janney compares the observed average flexural bond stress over a one-half beam length with values obtained at failure from the expression

$$u = \frac{V}{\Sigma o\, jd}$$

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He says that "as would be expected, this expression which is generally used in ordinary reinforced concrete design to determine bond stresses does not give an accurate picture of the flexural bond stress conditions in the prestressed test beams."

Indeed, it follows from a simple theoretical consideration that the above formula is not applicable to prestressed concrete beams if V denotes the *total* shear force at failure, but there is no reason why it should not be applicable if a reduced value of V is taken into account which corresponds to the *increase* of stress in the wire from the effective prestress to the stress at failure.

For the purpose of calculating bond stresses in prestressed concrete beams the load has to be divided into two parts. The first part corresponds to the margin between the effective precompression and zero stress at the bottom fiber. The bond stresses necessary to carry this part of the load are already effective in the beams; they transfer the prestress from the wires into the concrete and cannot be measured *after* the application of the prestress. Once the load has been reached which reduces the bottom fiber stress to zero, the beam behaves for any *additional* load in the same way as a nonprestressed beam. This additional load, but this only, has to be carried by bond stresses yet to be developed. The bond stresses measurable at a loading test, hereinafter called "reduced bond stresses," correspond therefore to this second part of the load only.

Since the losses of the prestress are not given in Mr. Janney's paper, it is not possible to calculate the reduced shear force and the corresponding reduced bond stress accurately, but an approximation can be obtained by ignoring the losses. Table A compares the last column in Mr. Janney's Table 3 with the reduced bond stress calculated as follows:

$$\frac{V}{\Sigma o\, jd} \times \frac{\text{increase in tensile stress}}{\text{prestress} + \text{increase in tensile stress}} = \text{Column 10} \times \frac{\text{Column 7}}{\text{Column 3} + \text{Column 7}}$$

It will be seen that the reduced values are either equal to or smaller than the values in the last column in Table 3. The maximum difference occurs in beam 5-c and amounts to 22.5 percent but this is an exceptional case. If the losses of prestress were taken into account, the reduced calculated values would be increased accordingly and the agreement would probably be even better. Some of the calculated values would be higher, some still lower than the values in the last column of Table 3, as would be expected if a formula represented the true behavior of a structure.

While the agreement between the modified Eq. (9) and the test results is satisfactory, it seems to be necessary to discuss the significance of the figures thus obtained.

TABLE A—COMPARISON OF BOND STRESSES

Beam No.	Average flexural bond stress over ½ beam length, psi	Reduced calculated average bond stress, psi
2-c	81	77
3-c	47	47
3-r*	195	185
4-c	28	28
5-c	40	31
5-r	102	94
6-c	69	68
6-r	244	205
7-cs	75	68

*The reinforcement of this beam is described in Table 3 as "clean" which is an obvious error.

The beams summarized in Table 3 failed either by steel fracture or by bond slip. It is obvious that in the case of steel fracture the average bond stresses do not represent average bond strengths but may be lower since the bond was not destroyed at failure and wires of higher strength might have developed higher bond stresses. In the case of bond slip, the difference between cracking load and ultimate load was small, varying between 3 and 10 percent. Since "the beams were loaded to failure in a few minutes without interruption" it seems to be doubtful whether this small difference is a real one. As established by Rüsch,[3] the bond strength obtained from tests depends on the rate of loading. It is quite possible, and in the writer's opinion even probable, that in all beams with clean wires the margin between cracking load and ultimate load would have been reduced had the cracking load been maintained for a few minutes. Indeed, it is feasible that all these beams would have failed at the cracking load if this load had been sustained for a sufficiently long period.

If cracking load equals ultimate load, the bond stresses obtained from the reduced shear correspond to the margin between zero stress at the bottom fiber of the beams and the tensile strength of the concrete. As long as the beam remains uncracked the increase in bond stress is negligible. When the beam cracks there is a transfer of tension into the wires, *i.e.*, a sudden increase in the bond stress to a level which if maintained for a certain period destroys the bond. Thus with this mode of failure the bond stress obtained may not be a characteristic figure either; it may be greater than the stress necessary for pulling out the wires. It would appear that reliable values for the bond strength of clean wires in prestressed beams could only be obtained in experiments on beams with artificial cracks in which there is a gradual increase of bond stress from the moment when the bottom fiber stress is zero and in which the rate of increase of the loading is very slow.

However, the establishment of such values would be of pure academic interest. Failure coinciding or nearly coinciding with the cracking load can obviously not be tolerated. In fact, it should be the basic principle of the design and execution of prestressed concrete that the possibility of failure by bond slip is excluded altogether even at a load much higher than the cracking load. In the writer's opinion this is the most important requirement of prestressed concrete structures from the point of view of safety since bond slip is always sudden without any warning. It has been proved by a great number of tests in many countries that this mode of failure can be avoided even without rusted wires. In design based on this principle, Eq. (9) need not be used at all. On the other hand, the low values obtained in the tests with clean wires (28 to 81 psi) show that a calculated value, however low the "permissible bond stress," is no guarantee against bond failure.

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By N. W. HANSON*

A discussion of some exploratory tests involving dynamic loading may be presented to amplify and confirm the author's findings regarding the nature of bond in pre-tensioned prestressed concrete. His tests were restricted to beams and stress transfer prisms subject to static loading. The test data presented here concern ten beams and four stress transfer prisms subjected to dynamic loading. In addition, two beams were tested statically as control specimens.

BEAM TESTS

The test beams were 3 in. wide, 5 in. deep, and 72 or 84 in. long. All beams were prestressed with two 0.208-in. diameter wires pre-tensioned to 150,000 psi (Fig. A). The pre-tension force was transferred to the concrete when the cylinder strength was about 3500 psi. Flexural tests were started when the cylinder strength reached about 5500 psi at an age of about 28 days. The only variable incorporated, other than method of loading, was the surface condition of the wire: clean and rusted surfaces. The beams were instrumented to determine the average bond stress between gage points 12 in. apart in the 72-in. beams and 14 in. in the 84-in. beams (Fig. A).

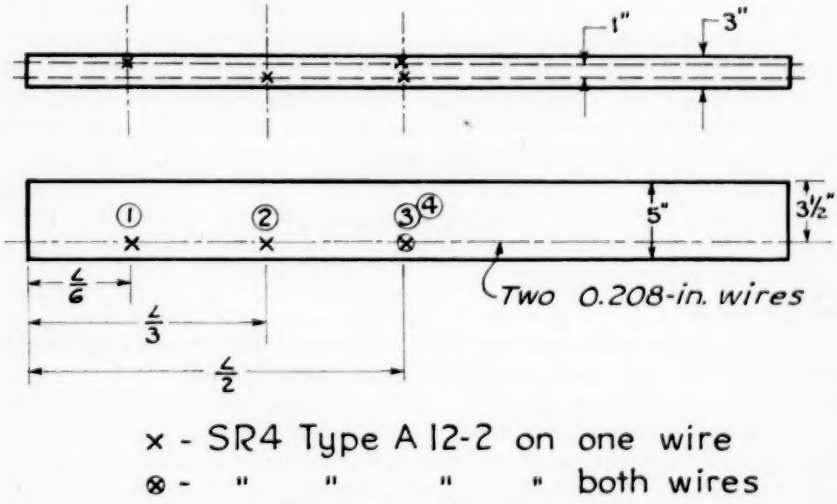

Fig. A—Test beams

*Assistant Development Engineer, Development Department, Portland Cement Assn., Chicago, Ill.

TABLE B—TEST RESULTS

Beam No.	Wire surface	Test method	Mode of failure	$\frac{M_{ult}}{bd^2 f'_c}$	Degree of loading,* percent	Loading above cracking,† percent	Load cycles to failure	Maximum observed bond stress, psi
1	rusted	static	wire fracture	0.216	100	100	—	210
2	clean	static	bond	0.186	86	90	—	183
3	rusted	repeated load	bond	0.169	76	33	170,000	185
4	rusted	repeated load	bond	0.168	77	35	283,000	182
5	clean	repeated load	bond	0.171	77	36	7,200	155
6	clean	repeated load	bond	0.153	73	26	654,000	130
7	rusted	impact	bond	0.174	86	61	20 drops 10 in. +1 drop 19 in.	145
8	rusted	impact	bond	0.178	88	67	5 drops 10 in. +4 drops 26 in.	150
9	clean	impact	bond	0.134	66	8	5 drops 7 in.	115
10	clean	impact	bond	0.131	65	4	5 drops 7 in.	not observed
11	clean	vibration	none	—	22‡	—	‡	28
12	clean	vibration	none	—	23‡	—	‡	29

*Moment at failure in percent of moment at wire fracture in static loading with rusted reinforcement, beam No. 1.
†Moment at failure minus cracking moment divided by the same difference for beam No. 1.
‡Sustained 10,000,000 cycles without noticeable detrimental effects.

Static tests

Beam No. 1 containing rusted wire reinforcement, and beam No. 2 with clean wire were tested statically with midspan loading over spans of 5 and 6 ft, respectively. As indicated in Table B, the beam reinforced with rusted wire failed by wire fracture, while the clean wire beam failed in bond. The flexural bond stresses observed near failure in both beams were of about the same magnitude as those reported by the author for clean wires. It should be noted, however, that the tensile strength of the steel was 200,000 psi and the ultimate elongation was only slightly in excess of 2 percent. With the high effective prestress of about 135,000 psi there remained only about 65,000 psi tensile stress to be developed by flexural bond before the steel fractured. Thus, the measured bond stress at wire fracture was only 210 psi, while the author reported bond strengths up to 1000 psi for rusted wires.

The relatively high effective prestress should also be considered in evaluating the data for the remaining beams in Table B. For example, the bond failure of beam No. 2 prestressed with clean wires occurred at a bending moment at which a bond stress of 183 psi was measured between the center strain gage and one 14 in. from the center. All other things being equal, if beam No. 2 had been prestressed with higher strength wire, it would have failed in bond at the same moment. A beam prestressed with rusted wire of the same higher strength, however, would probably have sustained a much higher moment than beam No. 1.

Repeated load tests

Two rusted wire beams and two clean wire beams were tested by repeatedly applying a load well above the cracking load until a bond failure occurred. Test results are given in Table B. The major significance of the repeated load data is expressed by the table column "Loading above cracking," which gives ultimate moment minus cracking moment divided by the same difference for beam No. 1 which failed by wire fracture. Beam No. 5 and 6 with clean wires indicate that the number of load cycles sustained before a bond failure took place decreased significantly when the loading above cracking

was increased from 26 to 36 percent. For beams No. 3 and 4 with rusted wires the repeated loading changed the mode of failure as compared to static loading from wire fracture to bond failure, and the ultimate moment was reduced about 23 percent. A comparison of beams No. 3, 4, and 5 shows that rusted wires sustained a number of load repetitions about 25 to 40 times that of clean wires for nearly equivalent loadings.

Repeated impact tests

Two rusted wire beams and two clean wire beams were subjected to impact loads by dropping a 32-lb weight from various heights at midspan of a 5-ft simple beam. Again the pertinent data are shown in Table B. The steel strains measured with the aid of a recorder at the gage points shown in Fig. A served as a basis for computing the percent of the static ultimate moment applied in the impact tests.

The rusted wire beams demonstrated good resistance to impact. The final stress level at each gage point at the weight drop just before bond failure was similar to that observed at failure in the static test of the beam prestressed with clean wire.

The results obtained from the tests of the clean wire beams were not so encouraging. Failure occurred at a moment slightly in excess of the cracking moment after only five weight drops in both tests.

As in the repeated load tests, flexural bond stresses built up at midspan and then moved toward the beam ends. Failure took place when a peak of flexural bond stress reached the prestress transfer region. Thus, flexural bond stresses, not the prestress transfer bond, was the primary cause of bond failure.

Vibration tests

It was felt that relatively high frequency, low amplitude vibrations might tend to decrease the prestress force in pre-tensioned wire by virtue of a gradual prestress transfer bond failure. Two clean wire beams were therefore placed over a 5-ft span and vibrated at frequencies of 940, 1200, 1440, and 1620 rpm by an eccentric rotating weight at midspan. Ten million cycles resulted in no significant changes in the steel strains along the length of wire.

PRISM TESTS

Four prisms 2 x 2 in. in cross section and 96 in. long were instrumented in a similar manner as those described by the author (Fig. B). A 0.208-in. diameter clean wire was placed in the center of the 2 x 2-in. cross section with a pre-tension of 150,000 psi. Cylinder strength of the concrete at time of stress transfer was 4350 psi. In one prism the wire was released slowly. The other three wires were cut while under tension, at distances of 1 in., 36 in., and 72 in. from an end of the prism specimen, with a resulting sudden transfer of stress. The stress transfer relationships, derived in the same manner as those presented by the author, indicated that there was little difference in the transfer length regardless of the manner of pre-tension release.

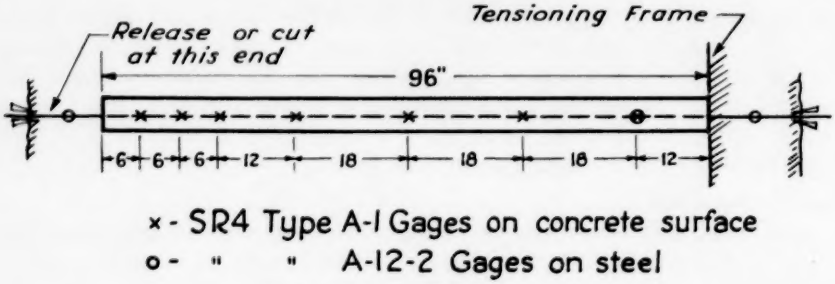

Fig. B—Test prisms (2 x 2 in.)

CONCLUDING REMARKS

The tests reported in this discussion were conducted to explore the effects of dynamic loading on the nature of bond in pre-tensioned prestressed concrete. The tests were limited in scope, and quantitative conclusions are not warranted. However, some general conclusions may be drawn to amplify those of the author. A serious loss of prestress due to primary failure of the prestress transfer bond did not take place in any of the dynamic tests reported —even for clean wire. Flexural bond stresses always increased sharply at cracking, and bond failures never took place before a concentration of flexural bond stress had built up at midspan and moved to the anchorage region near the beam ends. Thus, the prestress transfer bond never failed *before* the flexural bond had failed. *After* extensive failure of the flexural bond, however, the end anchorages by bond were unable to permit the beams to act as unbonded beams with mechanical end anchorages.

AUTHOR'S CLOSURE

The contributions of Dr. Hajnal-Kónyi, Dr. Abeles, and Mr. Hanson are valuable in adding further to an understanding of the nature of bond between pre-tensioned steel and concrete. It is interesting to note that each has made reference in various connections to the importance of the stress-strain properties and strength of the steel on the demands placed on bond in a particular member. For example, greater flexural bond resistance will be required to develop the full flexural strength of a beam reinforced with *untensioned* wire of 275,000 psi tensile strength than will be required in an identical member in which the steel is pre-tensioned to 150,000 psi. Furthermore, the flexural bond requirements of the latter beam will be greater than in a beam, again otherwise identical, with the wire reinforcement pre-tensioned to 150,000 psi but having a tensile strength of only 210,000 psi.

As Dr. Hajnal-Kónyi has pointed out, the tensile strength of most steels commonly used for prestressing could be developed fully in beams of normal steel percentage provided the bond is sufficient. However, this is seldom the case in practice and a normal tensile flexural failure manifests itself by concrete crushing after a flexural crack has widened sufficiently as a result

of local bond failure in the immediate vicinity of the crack.

Both Dr. Abeles and Dr. Hajnal-Kónyi have emphasized appropriately the undesirability of a bond failure as the ultimate mode of failure. A complete bond failure will always result in an ultimate strength below that which would result if the bond were adequate, and it will occur without warning. However, the author feels that increasing knowledge and additional research regarding the nature of flexural crack formation and its relationship to bond will ultimately result in the means for the designer to predict the bonding performance of steel and concrete for given design conditions.

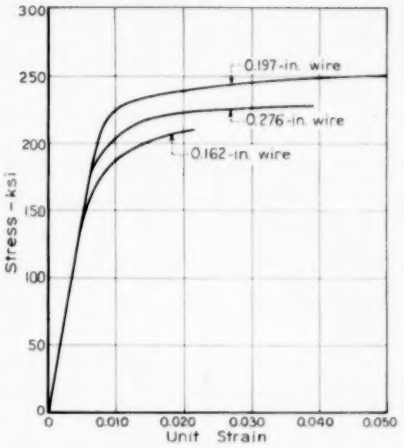

Fig. C—Stress-strain relationships for wires used in bond studies

Dr. Abeles' comments are well taken regarding the unsuitability for design purposes of the expression derived from an elastic analysis of the frictional bonding phenomenon. The author would like to reemphasize that the elastic analysis was merely presented to demonstrate the feasibility of the assumption that prestress transfer bond is a result of friction. Of course, the compressive and tensile stresses arising from the analysis are beyond the elastic range, thus the only value of the expression is to compare the general character of the curves shown in Fig. 3 with the experimentally derived curves.

Dr. Abeles has mentioned British tests in which 0.2-in. diameter wire performed satisfactorily from a bond standpoint when prestressed members were subjected to millions of repeated loads. Most writers discussing fatigue testing of prestressed concrete fail to note the extent of cracking at various stages of the test. Mr. Hanson's data show clearly that the fatigue resistance of bonded members is a function of the degree of load between the cracking load and the ultimate load which would prevail if the bond were satisfactory.

Dr. Abeles has quite correctly pointed out that the center-point load is undesirable if one is investigating the ultimate strength properties of beams. The purpose of the tests under discussion was to learn something of the character of bond and a center-point load was necessary to achieve some degree of crack control.

The stress-strain curves of the prestressing wires used in the beam tests are included in Fig. C.

Discussion of a paper by T. C. Powers:

Void Spacing as a Basis for Producing Air-Entrained Concrete*

By J. E. BACKSTROM, R. W. BURROWS, V. E. WOLKODOFF, and AUTHOR

By J. E. BACKSTROM, R. W. BURROWS, and V. E. WOLKODOFF†

Mr. Powers is to be commended for his valuable contributions to the technology of concrete. His theories concerning the void spacing factor are being investigated in the laboratories of the Bureau of Reclamation and have already been of value in the resolution of some previously anomalous data. The formulas developed for the spacing factor $\bar{L}$ are workable and attractive in that they employ values which are easily obtained by microscopical examination.

It is agreed that the air void system is probably the most important single factor influencing the frost resistance of concrete, but it is felt that more extensive data are required before an air void spacing factor of 0.01 in., or of any other magnitude, can be justifiably adopted as a design or a control criterion for frost resistant concrete.

The writers are cognizant of the fact that various organizations dealing with concrete technology will differ as to what constitutes "adequate" frost resistance. By Bureau of Reclamation standards, a concrete is considered to be adequately frost resistant if 500 or more cycles of freezing and thawing are required to obtain a 25 percent weight loss. All comments contained in this discussion are based on the above definition of adequate frost resistance.

Our data indicate that very appreciable increases in frost resistance can be obtained by reducing the spacing factor much below the recommended 0.01 in., and that, in our concretes, these lower spacing factors have been easily secured without exceeding the air contents recommended by ACI Committee 613. In addition the writers believe, and our data substantiate, that frost resistance is affected to a greater or lesser extent by variations in materials, curing, test methods and exposure, water-cement ratio, and air contents. Inasmuch as these effects are not constant it is difficult to visualize a single spacing factor which will assure adequate frost resistance in all cases. The scope of this presentation does not permit detailed discussion of all the variables; however, such will be undertaken in a series of papers proposed for future publication.

*ACI Journal, May, 1954, *Proc.* V. 50, p. 741. Disc. 50-46 is a part of copyrighted Journal of the American Concrete Institute, V. 26, No. 4, Dec. 1954, Part 2, *Proceedings* V. 50.

†Engineers and Engineering Petrographer, respectively, Bureau of Reclamation, Denver, Colo.

AIR VOID SIZE AND SPACING

Powers states, on p. 755, that "A limited amount of data indicate that the specific surface of the composite system of voids may be as low as 300 and as high as 800 in.$^{-1}$" Powers has concluded, apparently on the basis of Klieger's concretes, that the specific surface of the air voids in normal air-entrained concrete mixes is about 550 in.$^{-1}$ (sq in. per cu in. of air). That the relatively large size of these voids may not be typical of concrete in general is indicated by the results of the more than 170 determinations of specific surface (alpha) based on the entire system of voids, in air-entrained and vibrated concretes from both our laboratory and the field which have been made to date. Alpha values of less than 600 in.$^{-1}$ were found in only 5 percent of these cases, with the minimum value at 500 in.$^{-1}$ In the remaining concretes, values of alpha ranged from 600 to 1700 in.$^{-1}$ The air contents of these concretes were generally equal to those recommended by ACI Committee 613 for the various maximum size concretes involved, and, without exception, the spacing factors were below 0.01 in. with some as low as 0.003 in. In 17 concrete core specimens from Hungry Horse Dam, Montana, the alphas averaged 950 in.$^{-1}$ when entrapped air was excluded from the calculations and 750 in.$^{-1}$ for the entire system of voids; the corresponding spacing factors being approximately 0.005 and 0.006 in., respectively.

EFFECT OF SPACING FACTOR $\overline{L}$ ON FREEZING AND THAWING RESISTANCE

Fig. A, in which are plotted values obtained from Mr. Powers' Fig. 1, together with results of three unrelated investigations performed in the

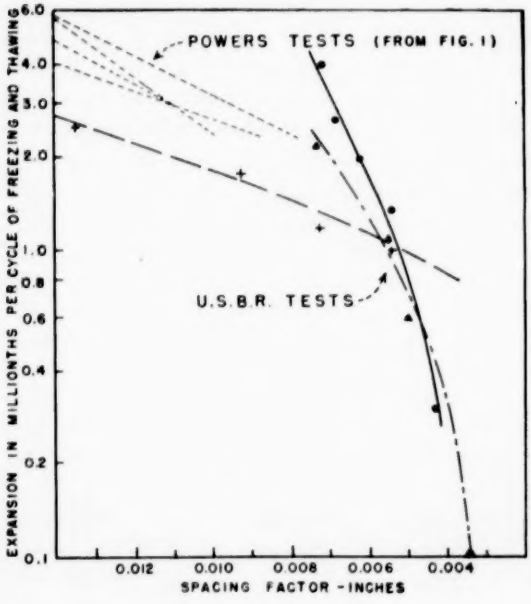

Fig. A—Effect of spacing factor on the frost resistance of different concretes. The three series represented contained different aggregates and cements of good quality

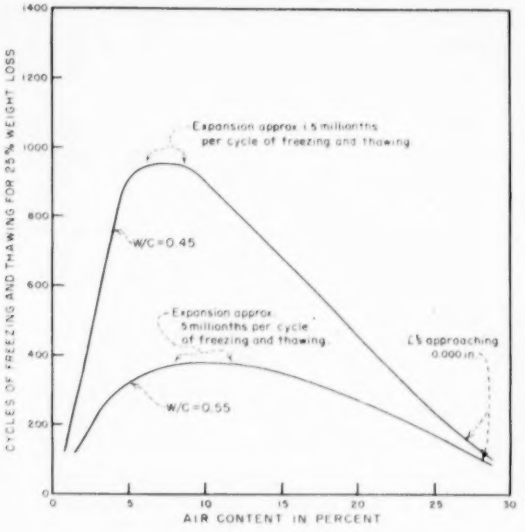

Fig. B—With this concrete, the water-cement ratio influenced the frost resistance at all air contents

Bureau laboratories which utilized different, but good quality materials, divest the $\overline{L}$ value of 0.01 in. of much of the importance attributed to this value by Mr. Powers. This is illustrated by curves in Fig. A which show progressive improvement in frost resistance accompanying a progressive reduction in $\overline{L}$ to values much lower than 0.01 in. Values of $\overline{L}$ as low as 0.0015 in. (not shown in Fig. A) have been obtained in laboratory concrete in which high resistance to frost action was developed. The point at which lower values of $\overline{L}$ fail to improve resistance to freezing and thawing as measured by weight loss has not yet been determined. However, some such limit is indicated from the shape of the curves on the righthand side of Fig. B. It should also be noted in Fig. A that the three different combinations of cements and aggregates produced different frost resistances at the same $\overline{L}$, showing that $\overline{L}$ was not the controlling factor in determining frost resistance in these cases.

Fig. B shows that the water-cement ratio greatly influences the frost resistance of the concretes over the entire range of air contents and presumably spacing factors. This is contrary to Powers' Fig. 2 which shows that the amount of freezable water* has little effect on the frost resistance at spacing factors below 0.01 in.

Fig. C represents tests on concrete containing a good quality aggregate and cement. According to our standards, a spacing factor of approximately 0.0085 in. is the largest which would produce adequate frost resistance for this particular concrete. Although this value differs from 0.010 in. by only 0.0015 in. the effect is actually very large as is indicated, within the limits of

*Freezable water is essentially that water in excess of that required for complete hydration of the cement.

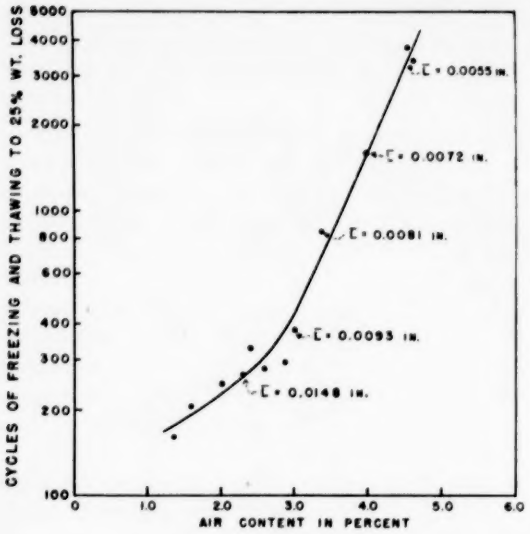

Fig. C—With this concrete, containing materials of good quality, a spacing factor of less than 0.0085 in. is indicated for good frost resistance

Fig. C, by relatively large changes in frost resistance resulting from small changes in $\bar{L}$ where $\bar{L}$ is less than 0.01 in.

ENTRAPPED AIR VOIDS

It has been our experience that the influence of compaction, water-cement ratio, addition of fines, and use of different air-entraining agents upon void size and distribution within a suite of concrete specimens is best seen if the measurements of the void system are computed on the basis of both the total void content (entrapped, natural, and entrained) and the composite voids (natural and entrained).

Until data are forthcoming from a series of tests now in progress to evaluate the definitive characteristics of entrapped air voids due predominantly to inadequate compaction, we have assumed from information available on several specimens of average concrete that any void larger than $\frac{3}{16}$ in. in any dimension and regardless of shape, or *any* irregular void represents entrapped air. Our observations indicate that entrapped air voids are predominantly irregular in shape. In poorly compacted concrete, entrapped air voids having a maximum dimension as large as 3 in. have been encountered; however, in most of our studies the entrapped air voids are found to range from $\frac{1}{2}$ to $\frac{3}{4}$ in. in size. In concrete from one structure the entrapped air voids were large but occurred sporadically; consequently, and even with methodical sampling, individual specimens contained a widely variable number of these large voids.

For any sample it has been our procedure to conduct the linear traverse on at least 70 to 100 sq in. of surface, usually on several slabs of concrete, and to complete from 150 to 250 in. of linear traverse, depending upon the

TABLE A—TYPICAL EFFECT OF ENTRAPPED VOIDS IN THE SPACING FACTORS OF THE VOID SYSTEMS IN COMPANION SPECIMENS OF CONCRETE

	Specimen A	Specimen B
Total air, percent	1.62	1.10
Entrained and natural air, percent	1.03	1.08
Entrapped air, percent*	0.59	0.02
Average chord intercept, entrained and natural voids, in.	0.0052	0.0051
Average chord intercept, total air including entrapped air, in.	0.0080	0.0052
Alpha, specific surface area, entrained and natural voids, in.$^{-1}$	769.2	784.3
Alpha, specific surface area, total air including entrapped air, in.$^{-1}$	500.0	769.2
Water-cement ratio	0.57	0.57
Maximum size aggregate, in.	6	6
Paste content, percent	15.12	15.12
Spacing factor $\overline{L}$, entrained and natural air voids, in.	0.0097	0.0094
Spacing factor $\overline{L}$, all voids including entrapped air voids, in.	0.0123	0.0095

*The entrapped air voids were of predominantly irregular shape and measured over $\frac{3}{8}$ in. in size.

maximum size aggregate, total air content, and amount of sample available for study. Depending on the grid employed by the microscopist to traverse the specimen with equidistant parallel lines, it is quite possible that all or nearly all the large entrapped voids on one surface are not intercepted whereas in a companion specimen of similar concrete a large proportion of the large voids is intercepted. The interception of a few large voids not characteristic of the natural void system exerts an influence on the void parameters out of all proportion to their relatively infrequent incidence, especially if these voids are intercepted along their maximum dimension.

It is believed that large entrapped air voids resulting from inadequate compaction do not significantly influence frost resistance of air-entrained concrete or mortar, but if intercepted during the traverse, they often radically decrease the indicated specific surface of the voids with a consequent increase in the spacing factor of the paste in the concrete being analyzed.

An example of the effect of entrapped air on the spacing factors for similar concrete is shown in Table A. Note that the difference in the spacing factors for specimen A is nearly 0.004 in., although the entrapped air voids intercepted constitute only about 2 percent of the total number of voids intercepted. Specimen B, in which entrapped voids were negligible, was taken several inches away from specimen A in the same sample. Approximately 175 in. of linear traverse on about 70 sq in. of surface were conducted on each specimen. This example, while not representative of normal concrete, was selected to emphasize the influence of a small number of large entrapped voids on the spacing factor and the difficulties sometimes encountered in sampling concrete. It is believed that the influence of spacing factor on frost resistance is better represented by the spacing factor obtained when the large entrapped air voids are excluded from the calculations. Our experience indicates that when spacing factors and other measurements of the void system are computed in this manner, especially in field concrete where effective control is to be evaluated, better and clearer relationships are obtained. Perhaps comparable results would be obtained if unduly long traverses are conducted on large samples to minimize the effect of larger entrapped voids on the spacing factor. However, this procedure, due to the length of

time involved under present methods of analysis, would be impractical and would make the determination of void spacing an unnecessarily arduous task.

Nonetheless, it should be pointed out that in this discussion the spacing factors presented in Fig. A and C are based on the entire system of voids; however, these concretes are considered to be well compacted and large entrapped voids were absent or occurred only infrequently.

POWERS' TABLES 2 AND 3

The writers have been unable to verify the figures contained in Tables 2 and 3. Mr. Powers' paper contains the statement (p. 748) that "Hence formula (A) applies when $\overline{L} = 0.01$ in. and the specific surface of the voids is 433 in.$^{-1}$ or higher." It appears to the writers that this statement contradicts a previous statement (p. 747) to the effect that formula (A) is to be used when the volume of paste does not exceed 4.33 times the total volume of the air voids. From Powers' previous publications it seems that the quoted statement is incorrect, that Tables 2 and 3 were calculated on the basis of this statement, and hence the values shown are incorrect.

CONCLUSIONS

Powers' spacing factor should be considered in evaluating frost resistant concrete; however, the writers believe that it should not, on the basis of present knowledge, be regarded as an unimpeachable criterion upon which to base the initial design or subsequent control of air-entrained concretes. The collection and analysis of additional data covering a wide range of materials, air contents, water-cement ratios, and methods of curing and testing may possibly define the limits within which the spacing factor *is* the prime controlling variable in the frost resistance of concrete.

AUTHOR'S CLOSURE

Verbal comments heard since publication of my paper revealed a tendency for some readers to feel that the paper condones or even encourages making concrete with high water-cement ratio paste. I did not intend to convey that impression. The paper deals only with the matter of deciding how much air to entrain in concrete to improve frost resistance. The conclusion reached is that for a considerable range of water-cement ratios entrained air will perform its function properly if the spacing factor is not over 0.01 in. For protection of questionable aggregate, for obtaining a high degree of impermeability, for high resistance to leaching, and for general excellence, a rich paste is to be preferred to a lean paste whether entrained air is used or not. Entrained air cannot make lean paste impart to concrete the same qualities that are imparted by a rich one. It can, however, make either grade of paste safe from frost attack.

Data presented by Messrs. Backstrom, Burrows, and Wolkodoff are significant and their interpretation merits careful consideration. Possibly

their conclusions will prove to be correct; that is, we may find that producing scientifically controlled, air-entrained concrete is a complex process of evaluating and controlling several variables. Such a possibility was recognized in one of the closing paragraphs of my paper.

I hope most readers will realize that I did not offer a single value for spacing factor as an "unimpeachable criterion on which to base the initial design or subsequent control of air-entrained concrete;" I am in full agreement with Messrs. Backstrom, Burrows, and Wolkodoff on this point—it probably is not. What I did try to present was ".... the next logical step in refining production of air-entrained concrete" (p. 742).

Although I shall not attempt to refute the discussers' conclusions—they may be right—it seems worthwhile to re-examine their data from a different point of view to show that another plausible interpretation is possible. I shall try to show that simply maintaining the proportion of air-entraining agent at a preselected value as proposed in my paper still appears to be the most practical way to obtain all benefits that can reasonably be expected from entrained air. Regarded as a stopgap, pending results of further research and field experience, my proposal seems more conservative and practical than trying, as at present, to maintain a constant total air content regardless of what may be causing the air content to vary.

Issues raised by Messrs. Backstrom, Burrows, and Wolkodoff include the question as to how to interpret laboratory freezing and thawing tests. What is the relationship between laboratory performance and field performance? When results from two different laboratories are not the same, which result gives the more reliable basis for estimating field performance? It seems to me that the discussers' conclusions rest rather heavily on the assumption that relative performance in a particular laboratory test is a direct indication of relative performance in the field.

In Fig. A we have data from two sets of freezing and thawing tests made on air-entrained concretes of similar qualities. These two sets of results demonstrate that the result depends on manner of testing; that is, it depends on conditions under which the specimens are frozen and thawed. If this is true, it is more than likely that had the test been made under *natural* conditions the indications would have been different from those of *either* of the two sets of data in Fig. A. It is pertinent, therefore, to inquire into features that account for differences in test results. It would be beyond proper scope of this discussion to present a study of this subject; I shall indicate only the outlines of it.

The principal features of the two test conditions may be compared with each other and with natural conditions as follows:

	USBR	PCA	Natural
Rate of cooling, deg F per hr	40	20	5 ± (maximum)
Lowest temperature, deg F	+10	−12	Various

If identical concretes are frozen under such different conditions, amounts of ice produced are greater the lower the lowest temperature. Hence, under Portland Cement Assn. conditions more ice is produced than under U. S. Bureau of Reclamation conditions. Under natural conditions, different amounts are produced, according to the weather. The *rate* at which the ice is produced depends upon the rate of cooling. Hence, rate of ice production is highest for USBR conditions and lowest for natural conditions.

We understand the mechanism of frost action well enough to make an intelligent guess as to how these differences in conditions affect performance of concrete and we can, on this basis, form an opinion as to what results of the PCA and USBR tests on similar concretes mean with respect to probable performance under natural conditions. Briefly stated, the indications are these:

(1) The USBR tests should be relatively more severe than either the PCA test or field conditions when concrete contains relatively low-grade paste not fully protected with entrained air. For such concrete, the rate of ice production rather than the amount produced should be the important factor.

(2) The USBR test should be somewhat less severe on air-entrained concrete made with high-grade paste than the PCA test and probably less severe than natural conditions if the natural freezing temperature is equally low. For such concrete the length of time the concrete remains at low temperature should be the important factor.

(3) The spacing factor at which low-grade, air-entrained paste shows the same resistance as high-grade, air-entrained paste should be smaller the higher the rate of freezing. Hence, the USBR tests should indicate a need for relatively more air and a smaller spacing factor for the leaner mixes than either the PCA test or field performance.

(4) Judged by its performance in concrete, a "sound" aggregate is one that does not become saturated to the critical degree during the course of seasonal wetting and drying of concrete. With this kind of aggregate in concrete exposed under usual natural conditions (not continuously submerged), protecting the paste with proper concentration of air bubbles should give permanent immunity to frost attack, but not to other disintegrative forces of nature.

(5) Even with sound aggregate, air-entrained concrete may eventually fail in a laboratory freezing and thawing procedure that does not provide interrupting freezing and thawing cycles with "seasonal drying." Failure is due to the gradual uptake of water by aggregate particles. (It may be due also to the gradual filling of air voids entrained in the paste.) Some aggregate particles become disruptive when they take up more than about 92 percent of their capacity for water if the surrounding paste prevents escape of the excess during freezing.

Thus laboratory tests that allow no drying after freezing and thawing cycles start gradually produce a vulnerable condition that is rarely if ever found in the field. This should be taken into account when interpreting a laboratory test.

(6) In the continuously wet laboratory cycle, failure due to uptake of water by the aggregate can, in some cases, be retarded by using an amount of entrained air higher than that required to protect paste. Replacing part of the surrounding paste with entrained air apparently enables some water to escape from aggregate particles during freezing and thus reduces disruptive pressure in the aggregate. Protection is not complete; entrained air does not prevent failure from soaked aggregate; at best, it retards it.

With respect to test interpretation, the significance of seasonal drying must again be pointed out.

(7) Aggregate particles larger than a certain size may fail when saturated and frozen, whether or not they are sealed by hardened paste. Critical size depends principally on the porosity, pore size, and strength. Hence, with certain aggregates the largest particles will fail on freezing after becoming saturated and the effect of this cannot be modified with entrained air. The life of concrete containing such particles depends on those conditions that control uptake of water by aggregate particles.

DISCUSSION OF FIG. A

Let us now examine Fig. A with these considerations in mind.

If concrete contained no air voids at all, practically no water could escape from the paste during freezing and the least possible amount of expansion would be equal to 9 percent of the volume of water frozen. When voids are present, some of the water escapes from the paste and dilation is correspondingly reduced. It can be shown on the basis of reference 2* that the reduction in expansion due to escape of water from the paste is an inverse function of the thickness of the webs of paste between the air bubbles. For that reason, in Fig. 1, I plotted expansion against reciprocal of the spacing factor, $1/L$. To retain the perspective afforded by Fig. 1, I plotted the USBR data given in Fig. A along with PCA data in Fig. D. It was necessary to enlarge the graph to accommodate USBR data. Having done this I was able to add three points from the PCA data that were originally omitted from Fig. 1.

As pointed out in the paper, the four different kinds of concrete represented by the PCA data all exhibit about the same degree of frost resistance at any spacing factor smaller than 0.01 in. As shown in Fig. D the USBR data for three different concretes have the same indication qualitatively but they have a different indication quantitatively; differences in frost resistance at a given spacing factor may be considered insignificant only when the spacing factor is smaller than about 0.005.

This difference between two sets of data can be explained in terms of differences in rate of freezing. In the PCA laboratory, rate of freezing is about 20 deg F per hr; in the USBR laboratory it is about 40 deg F per hr. It is in line with theoretical considerations already mentioned that curves for the different mixes converge at a smaller spacing factor the higher the rate of freezing.

*References in this discussion are those listed at the end of the original paper.

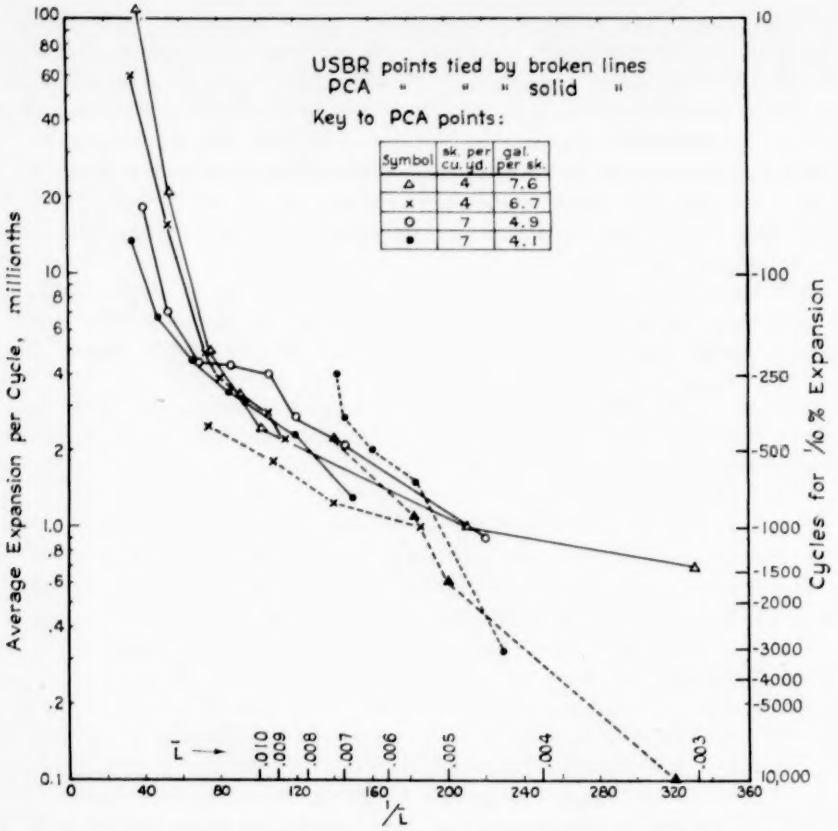

Fig. D—Relationships between rate of expansion and spacing factor from USBR and PCA data

Under natural conditions, rate of cooling is less than 5 deg F per hr. We should expect, therefore, that under natural conditions the spacing factor might be the dominant variable at values even larger than the limit indicated by PCA tests—that is, larger than 0.01 in.

The basis for suggesting 0.01 in. as the upper limit of spacing factor was not fully explained in the paper. Emphasis was on the point that if the spacing factor were that small or smaller, the same factor could be used for concretes of different characteristics. Suggesting 0.01 in. as the upper limit was based on three different considerations: (1) the known effect of entrained air on the expansion of paste; (2) the special curing conditions represented by the data in Fig. A or D; (3) findings reported by Klieger[7] as to optimum spacing factor for specimens dried once before starting freezing and thawing.

As reported in reference 3, dilation of air-entrained pastes can be totally eliminated by means of entrained air. Direct measurements on neat cement pastes of various water-cement ratios containing different amounts of entrained

air showed that expansion did not occur when the spacing factor of bubbles was not over 0.01 in. At such a spacing factor freezing caused pastes to contract rather than dilate, the contraction being due to extraction of water from gel by ice crystals in air voids. It is significant that reducing the spacing factor below 0.01 in. gave little additional modification of the effect of freezing. Thus, we have experimental basis for believing that cement paste is adequately protected by entrained air when the spacing factor is not over 0.01 in.

The special curing condition referred to was that specimens in both USBR and PCA tests represented in Fig. A or D were cured continuously moist and not given a period of drying before starting freezing and thawing, or at any later time during the test. For this condition our results agree with USBR results and support the conclusion pointed out by Messrs. Backstrom, Burrows, and Wolkodoff that the smaller the spacing factor the higher the resistance to freezing and thawing. In Fig. D only one of the PCA points departs significantly from the general downward trend. This departure might be due to experimental error, for that point represents a specimen containing over 12 percent air and thus one in which spacing factor was difficult to measure accurately.* To reconcile this trend with what has just been reported about effect of entrained air on dilation of neat paste, we must make the reasonable assumption that damage caused by freezing when the spacing factor is less than 0.01 in. is caused by water freezing in aggregate particles and not by the water in paste (see statement 5, p. 760-8). The fact that entrained air modified the effect of freezing in aggregate particles shows that the largest particles in this particular aggregate were below the critical size for that material, or else the largest particles did not become fully saturated during the test (see statements 6 and 7, p. 760-9).

We have good reason to believe that air-entrained concrete normally develops a higher degree of frost resistance than is indicated by data in Fig. D; this is the third factor mentioned above. Klieger's data show that preliminary drying adds considerably to frost resistance. In specimens dried for 2 weeks and then soaked 3 days before beginning freezing and thawing tests, no benefit was obtained by reducing the spacing factor much below 0.01 in. An explanation for this is that once a sound aggregate becomes dried after the concrete is hard, it cannot readily be resaturated to the critical degree. Hence, the beneficial effect of reduced spacing factor described in connection with Fig. D did not appear with those predried specimens.

Thus, my recommendation of 0.01 in. for upper limit was based on the assumption that benefits from smaller spacing factors are marginal and would be found in the field only under rare conditions.

Some of the above remarks may seem to be at odds with the results reported by Brewer and Burrows in 1951.† They showed that dried specimens may have less frost resistance than those continuously moist-cured. However,

*It might be due also to a limitation of our way of computing spacing factor. The voids in the USBR specimen were much finer than those in the PCA specimen, i.e., the specific surfaces differed widely.

†Brewer, H. W., and Burrows, R. W., "Coarse-Ground Cement Makes More Durable Concrete," ACI JOURNAL, Jan. 1951, *Proc.* V. 47, p. 353.

their results were obtained from non-air-entrained concrete, and resistances were all relatively low, less than 300 cycles for failure. Klieger's data on air-entrained concrete seem to be the most pertinent information now at hand.

Thus, it seems that the data of Fig. A can be reconciled with other indications that air-entrained concrete can be produced on the basis of a single limit on spacing factor applicable to all practical grades of concrete, and that the upper limit need not be below 0.01 in., save possibly under unusual circumstances. Apparent need for different spacing factors for different mixes seems to be an effect of the high rate of freezing used in laboratory tests. Benefit from lower spacing factors is obtained only after a long period of continuous submersion, and this benefit could not be expected with all aggregates.

DISCUSSION OF FIG. C

This figure shows that reducing the spacing factor below 0.01 in. certainly reduced rate of surface crumbling under freezing and thawing and it is clear that still smaller spacings would have been beneficial, as pointed out by the discussers. For reasons already given, it is questionable whether improvement from reducing factor below about 0.01 in. would be realized under field conditions. This diagram is based on number of cycles of freezing and thawing required for a 25 percent weight loss, rather than on dilation per cycle. On inquiry, I learned that USBR experience is the same as ours, namely, that when frost resistance of air-entrained concrete is upwards of 500 cycles, freezing produces little if any dilation. Additional cycles cause surfaces to crumble slowly while the interior of the specimen remains sound. Surface loss is probably due in part to leaching of lime from paste and in part to absorption of water by aggregate particles (see statement 5, p. 760-8). It seems questionable whether entrained air should be used on the basis of requirements for retarding surface crumbling induced primarily by continuous submersion. Such conditions in the field are too rare to be a justifiable basis for mix proportioning.

However, if low spacing factors can be produced without undue loss of strength, they might well be used, even though possible extra benefits under field conditions might be of doubtful value. It is interesting to learn that the U. S. Bureau of Reclamation is producing air-entrained concrete in which spacing factor at a given air content is unusually low as compared with air-entrained concretes with which we are familiar. This seems to point up one of the principal conclusions of my paper—different mixes and materials may produce significantly different spacing factors at the same air content.

DISCUSSION OF FIG. B

Data in this figure do not seem pertinent to the question of selecting a spacing factor for air-entrained concrete. From the point of view adopted for this discussion, we see here a case of failure that cannot be controlled with

entrained air. Failure might be due to aggregate particles exceeding the critical size; that is, the largest particles fail when saturated, regardless of amount of entrained air. Before the largest particles become fully saturated, entrained air is able to modify the effect of freezing in aggregate particles. The paste probably was fully protected at an air content below 5 percent.

Superior performance of the concrete made with the better paste (W/C = 0.45) was probably due to ability of this paste to retard uptake of water by aggregate particles. Notice that the criterion for failure is weight-loss. Possibly at the higher air contents, damage was nearly all superficial.

Bad effects of very high air contents should not be surprising to anyone who has examined such concrete microscopically. With 5 percent air, about 1/5 of the space around aggregate particles is occupied by air, 4/5 by paste; with 25 percent air, volume of air is at least equal to volume of paste. Normally, a paste air-bubble mixture would be described as froth with any air contents above about 8 percent of concrete volume. Such froth has impaired strength and probably little ability to keep water from penetrating either the froth or the aggregate.

It seems doubtful that performance under conditions where freezing and thawing cycles are interrupted by seasonal drying could be predicted from this continuously wet freezing and thawing test. Probably, under natural freezing conditions, effects of differences in W/C would not appear if the spacing factor did not exceed 0.01 in. in either case.

Proper limit for spacing factor cannot be fixed by considering performance of this kind of concrete under these test conditions, unless control of surface crumbling when frozen and thawed continuously submerged is definitely needed.

Finally, I must agree with Messrs. Backstrom, Burrows, and Wolkodoff that on p. 748, the last sentence of the middle paragraph should read: "Hence, formula (B) applies when $\bar{L}$ = 0.01 in. and the specific surface of the voids is 433 in.$^{-1}$ or higher." Moreover, Table 2 is incorrect; the two formulas were switched, B being used for the first three columns, whereas A should have been used, and vice versa for the rest of the columns. The table has been recalculated, and the correct result is presented herewith.

The error shows up at several points in the paper where values obtained from Table 2 were used. Fortunately, the effect is not important. For example, on p. 756, second line of the first complete paragraph, an increase from 4.8 to 7.0 percent is mentioned, whereas the figures should have been 4.0 and 6.0 percent. Information conveyed by the sentence is not appreciably distorted by incorrect figures.

Table B is presented to serve those who may be satisfied to base proportioning and control on a single spacing factor but wish to use a factor lower than the maximum permissible for paste protection. It is based on a factor of 0.006 in. The table shows that such a factor is not practical unless specific surface of voids can be kept above 700 in.$^{-1}$

TABLE 2—AMOUNTS OF AIR REQUIRED FOR 0.01-IN. SPACING FACTOR (corrected)

Sacks per cu yd	Air requirement, percent of concrete volume, for voids having specific surfaces (in.$^{-1}$) indicated								
	300	350	400	450	500	550	600	700	800
4 gal. per sack									
8	10.0	8.5	7.5	6.4	5.0	4.1	3.4	2.4	1.8
7	8.7	7.5	6.6	5.6	4.4	3.6	3.0	2.1	1.6
6	7.5	6.4	5.6	4.8	3.8	3.1	2.5	1.8	1.3
4½ gal. per sack									
8	10.1	9.1	8.0	6.8	5.4	4.4	3.6	2.6	1.9
7	9.3	8.0	7.0	6.0	4.7	3.8	3.2	2.2	1.6
6	8.0	6.8	6.0	5.1	4.0	3.1	2.7	1.9	1.4
5 gal. per sack									
8	11.2	9.6	8.5	7.2	5.7	4.7	3.8	2.7	2.0
7	9.8	8.5	7.4	6.3	5.0	4.1	3.4	2.4	1.8
6	8.5	7.2	6.4	5.4	4.3	3.5	2.9	2.0	1.5
5	7.1	6.0	5.3	4.5	3.6	2.9	2.4	1.7	1.2
5½ gal. per sack									
8	11.9	10.2	8.9	7.6	6.0	5.0	4.0	2.9	2.1
7	10.4	8.9	7.8	6.7	5.3	4.3	3.5	2.5	1.9
6	8.9	7.6	6.7	5.7	4.5	3.7	3.0	2.2	1.6
5	7.5	6.4	5.6	4.8	3.8	3.1	2.5	1.8	1.3
6 gal. per sack									
8	12.6	10.8	9.4	8.1	6.4	5.2	4.3	3.0	2.2
7	11.0	9.4	8.3	7.1	5.6	4.6	3.8	2.7	2.0
6	9.4	8.1	7.1	6.1	4.8	3.9	3.2	2.3	1.7
5	7.9	6.8	5.9	5.1	4.0	3.3	2.7	1.9	1.4
4	6.3	5.4	4.7	4.0	3.2	2.6	2.1	1.5	1.1
6½ gal. per sack									
8	13.2	11.3	8.9	8.5	6.7	5.5	4.5	3.2	2.4
7	11.6	8.9	8.7	7.4	5.9	4.8	3.9	2.8	2.1
6	8.9	8.5	7.4	6.4	5.0	4.1	3.4	2.4	1.8
5	8.3	7.1	6.2	5.3	4.2	3.4	2.8	2.0	1.5
4	6.6	5.7	5.0	4.3	3.4	2.8	2.3	1.6	1.2
7 gal. per sack									
8	13.9	11.9	10.4	9.0	7.0	5.8	4.7	3.4	2.5
7	12.2	10.4	9.1	7.8	6.2	5.0	4.1	2.9	2.2
6	10.4	8.9	7.8	6.7	5.3	4.3	3.6	2.5	1.9
5	8.7	7.5	6.5	5.6	4.4	3.6	3.0	2.1	1.6
4	6.9	6.0	5.2	4.5	3.5	2.9	2.4	1.7	1.2
7½ gal. per sack									
8	14.5	12.4	10.9	9.4	7.4	6.0	5.0	3.5	2.6
7	12.7	10.9	9.6	8.2	6.4	5.3	4.3	3.1	2.3
6	10.9	9.3	8.2	7.0	5.5	4.5	3.7	2.6	1.9
5	9.1	7.8	6.8	5.8	4.6	3.8	3.1	2.2	1.6
4	7.3	6.2	5.5	4.7	3.7	3.0	2.5	1.8	1.3

TABLE B—AMOUNTS OF AIR REQUIRED FOR 0.006-IN. SPACING FACTOR

Sacks per cu yd	Air requirement, percent of concrete volume, for voids having specific surfaces (in.$^{-1}$) indicated								
	450	500	550	600	700	800	900	1000	1200
4 gal. per sack									
8	11.1	10.0	9.1	8.3	7.1	5.6	4.3	3.4	2.3
7	9.7	8.7	8.0	7.3	6.2	4.9	3.7	3.0	2.0
6	8.3	7.5	6.8	6.2	5.3	4.2	3.2	2.5	1.7
4½ gal. per sack									
8	11.8	10.6	9.6	8.8	7.6	5.9	4.5	3.6	2.4
7	10.3	9.3	8.4	7.7	6.6	5.2	4.0	3.1	2.1
6	8.8	8.0	7.2	6.6	5.7	4.4	3.4	2.7	1.8
5 gal. per sack									
8	12.5	11.2	10.2	9.4	8.0	6.3	4.8	3.8	2.5
7	11.0	9.9	9.0	8.2	7.0	5.5	4.2	3.4	2.2
6	9.4	8.5	7.7	7.0	6.0	4.7	3.6	2.9	1.9
5	7.8	7.0	6.4	5.9	5.0	3.9	3.0	2.4	1.6
5½ gal. per sack									
8	13.2	11.9	10.8	9.9	8.5	6.6	5.1	4.0	2.7
7	11.5	10.4	9.5	8.7	7.4	5.8	4.5	3.5	2.4
6	9.9	8.9	8.1	7.4	6.4	5.0	3.8	3.0	2.0
5	8.2	7.4	6.8	6.2	5.3	4.2	3.2	2.5	1.7
4	6.6	6.0	5.4	5.0	4.2	3.3	2.6	2.0	1.4
6 gal. per sack									
8	14.0	12.6	11.4	10.5	9.0	7.0	5.3	4.3	2.8
7	12.2	11.0	10.0	9.2	7.9	6.2	4.7	3.8	2.5
6	10.5	9.4	8.6	7.9	6.8	5.3	4.1	3.2	2.1
5	8.7	7.9	7.2	6.6	5.6	4.4	3.4	2.7	1.8
4	7.0	6.3	5.7	5.2	4.5	3.5	2.7	2.1	1.4
6½ gal. per sack									
8	14.7	13.3	12.0	11.1	9.5	7.4	5.7	4.5	3.0
7	12.9	11.6	10.5	9.6	8.3	6.5	5.0	3.9	2.6
6	11.0	9.9	9.0	8.3	7.1	5.6	4.3	3.4	2.2
5	9.2	8.3	7.5	6.9	5.9	4.6	3.5	2.8	1.9
4	7.4	6.6	6.0	5.5	4.7	3.7	2.8	2.3	1.5
7 gal. per sack									
8	15.4	13.9	12.6	11.6	9.9	7.8	6.0	4.7	3.1
7	13.5	12.2	11.0	10.2	8.7	6.8	5.2	4.1	2.7
6	11.6	10.4	9.5	8.7	7.4	5.8	4.5	3.5	2.4
5	9.6	8.7	7.9	7.2	6.2	4.9	3.7	3.0	2.0
4	7.8	7.0	6.3	5.8	5.0	3.9	3.0	2.4	1.6
7½ gal. per sack									
8	16.2	14.6	13.2	12.2	10.4	8.1	6.2	5.0	3.3
7	14.1	12.7	11.6	10.6	9.1	7.1	5.5	4.3	2.9
6	12.1	11.2	9.9	9.1	7.8	6.1	4.7	3.7	2.5
5	10.1	9.1	8.3	7.6	6.5	5.1	3.9	3.1	2.0
4	8.1	7.3	6.6	6.1	5.2	4.1	3.1	2.5	1.6

Discussion of a paper by D. F. Billet and J. H. Appleton:

Flexural Strength of Prestressed Concrete Beams*

By P. W. ABELES, STEPHEN REVESZ, and AUTHORS

By P. W. ABELES†

On his visit to Urbana in 1952 the writer had an opportunity of seeing particulars of the tests and discussing with the authors their investigations. Their intention was to provide general equations for the analysis of the ultimate load conditions of under-reinforced and over-reinforced prestressed concrete beams with bonded steel. The writer expresses full agreement with the authors' general description of the modes of failure and their second conclusion. However, there are a number of other points which indicate that the test results refer to a special type of beam with somewhat limited bond resistance and thus differ from the behavior of prestressed concrete beams with well bonded wires, as always occurs with pre-tensioned prestressing steel and sometimes with well-bonded post-tensioned cables. With pre-tensioning, good bond is essential to avoid noticeable slip; otherwise the structure could not be considered properly prestressed. With post-tensioning, where good bond is preferable but not essential, bond can be ensured only if there is sufficient space to inject the grout and the latter has sufficient strength.

During the last 10 years the writer has carried out many failure tests on prestressed beams with pre-tensioned wires[1-5] and has also investigated beam tests on post-tensioned steel.[6] These investigations have indicated somewhat different results with regard to failure conditions. The tests described in references 1 to 5, as well as numerous acceptance tests, have shown also some difference with regard to cracking as compared with the authors' description. This may be explained from the properties of their test specimens. According to Fig. 1 the specimens contained post-tensioned wires in a sufficiently large duct to allow satisfactory grouting; but the loading tests were carried out 2 to 3 days after grouting, that is, at an age when the strength of the grout was relatively low. Although, as reported, the test results indicated that all portions of the previously rusted wires were bonded, it is impossible that, in view of the limited concrete strength, this bond could have been so effective as if the test had taken place later when the grout had obtained greater strength.

*ACI Journal, June 1954, *Proc.* V. 50, p. 837. Disc. 50-49 is a part of copyrighted Journal of the American Concrete Institute, V. 26, No. 4, Dec. 1954, Part 2, *Proceedings* V. 50.
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First the question of cracking may be discussed. It is important to distinguish between microscopic and visible cracking. Prof. R. H. Evans has found microscopic cracks 1/15,000 in. wide long before they became visible.[7] The writer recently had an opportunity of noticing apparent microscopic cracks from electric strain gage readings obtained for pre-tensioned wires in tests being made by the research department of British Railways. In this case it appeared that microscopic cracks occurred at a concrete tensile stress of 550 psi while visible cracks did not develop before a stress of approximately 1100 psi. From the writer's publications[1-5] it is seen that in prestressed beams and slabs with well distributed pre-tensioned wires, visible cracking occurred at a concrete tensile stress of approximately 1000 to 1100 psi for a concrete of a cube strength of 6500 to 9000 psi, corresponding to a cylinder strength of about 4000 to 7500 psi. However, with beams in which the tensioned steel is not located close to the tensile fiber but concentrated in cables, cracks will become visible at a somewhat lower tensile stress, that is, at 850 to 1000 psi. In the authors' tests much lower values were obtained. If the test results are compared for 4000 to 7500 psi concrete cylinder strengths, the tensile stress at cracking varies between 565 psi for beam B18 and 809 psi for beam B21. It would be interesting to know whether these values relate to microscopic cracks obtained from strain gage readings (for which they are relatively high) or to visible cracking (for which they are relatively low).

The writer was greatly interested in the fourth conclusion which indicates that good agreement was obtained between the cracking stress and the modulus of rupture for two-point loads on a prism 6 x 6 in. of 18-in. span. Generally, modulus of rupture tests on different sizes of specimens with different loadings show great discrepancies between individual results. However, they are only arbitrary values for an assumed stress distribution; for example, in all the cases described in references 1 to 5 the modulus of rupture varied between 700 and 900 psi, though visible cracking did not occur before a stress of 1000 psi. In this case the specimens were 4 x 4 x 16 in. with a point load at the center.

It would appear that in the authors' tests such good agreement was obtained by the coincidence that both test beams and specimens for modulus of rupture were 6 in. wide. If the qualities of concrete with cylinder strengths between 5000 and 8200 psi are considered, the ratios vary between 7.03 and 13.55. This differs greatly from the figures given by Price,[8] where the ratios for these strength values are stated as varying between 7.4 and 8.6; that is, the modulus of rupture, corresponding to a cylinder strength of 5000 psi, is 675 psi and that of 8000 psi, 930 psi. In Table 2, however, the highest modulus of rupture of 805 psi relates to a concrete of 5650 psi cylinder strength, while the concrete of the highest cylinder strength (8200 psi) has a modulus of rupture of only 605 psi. The writer would be obliged if the authors would give an explanation as to how these variations were obtained.

With regard to the cracking load, it may be added that several hundred acceptance tests were made by the civil engineering department, British Railways Eastern Region, on precast prestressed beams 12 to 102 ft long. The acceptance load corresponded to a concrete tensile stress of 750 to 800 psi for pre-tensioning and 650 psi for post-tensioning. No cracks occurred under this loading except for a few individual instances apparently due to manufacturing faults. In one particular instance the test load corresponding to a tensile stress of 775 psi was sustained for 30 days, during which time the deflection increased but no visible cracks developed. When comparing this with the authors' tests there are quite a number of cases where cracking took place below a stress of 650 psi, which would correspond to successful acceptance tests on prestressed beams 78 to 102 ft long with post-tensioned well bonded cables.

With regard to ultimate load conditions, two approaches are shown by the authors, that is, stress relationship in Eq. (1) to (4) and strain considerations in Eq. (5) to (8). The combination of these two groups of equations is, however, justified only if the strain ϵ_{su}, which is assumed to correspond to the ultimate steel stress $f_{s\,ult}$, represents the real *maximum* strain in the steel at failure. When the bond is destroyed between the cracks at failure the strain ϵ_{su} will equal the maximum steel strain. However, if the steel is well bonded to the concrete, the bond is destroyed only in the immediate neighborhood of the cracks, and nobody has been able so far to measure the steel strain in a crack. Consequently, the strain conditions of well bonded steel are entirely unknown, while those of less well bonded wires apparently agree well, as the authors' tests have proved.

In reference 6 the writer investigated a great number of test results on the following simple approximation, based solely on strength properties of the materials, the strain relations being disregarded entirely. Fig. A shows an adjustment of the basic stress distribution to the authors' Fig. 4, where

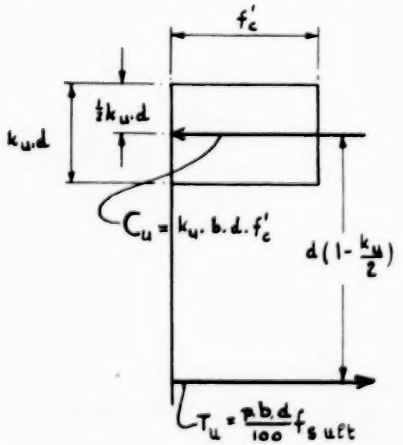

Fig. A—Simplified stress distribution at failure for well bonded steel

$k_1 = 1$, $k_2 = \frac{1}{2}$, and $k_3 = 1$, and thus the maximum possible is obtained, which can be achieved only if there is excellent bond. Otherwise reduction factors have to be considered. The writer has used the following equations:

$$M_{ult} = p\,(1 - vp)\,\frac{bd^2}{100} f_{s\,ult} = 2\,vp\,(1 - vp)\,bd^2\,f_c'$$

where $f_{s\,ult}$ is the actual tensile strength of the steel, $p = (100\,A_s)/\text{bd}$ the percentage, and the strength ratio $v = f_{s\,ult}/(200\,f_c')$. This results in $k_u = 2\,vp$. It was considered a safe assumption to limit k_u by $\frac{1}{2}$ to obtain a balanced design, resulting in $M_{max} = \frac{3}{8}\ (\text{bd}^2\ f_c')$. This corresponds to a limit percentage for balanced design of

$$\bar{p} = \frac{1}{4v} = \frac{50\,f_c'}{f_{s\,ult}}$$

The main difference between these formulas and those of the authors' is that here the ultimate strength of the steel is taken into account and not the stress f_{su}, corresponding to the maximum strain ϵ_{su} as suggested by the authors.

Fig. B shows test results investigated by the writer in reference 6, indicating the value J which is the ratio between the actual failure load bending moment and the value calculated according to the above formulas. J also represents the ratio between calculated steel stress and steel strength. All these test results are plotted for ratios of percentages $p/\bar{p}$, that is, the ratio of the percentage to that for balanced design. It is seen that almost all values are above the theoretical line $J = 1$ for $p/\bar{p} \leqq 1$. There are only a few test results of nonbonded specimens with results of $J < 1$, which was expected, when the steel can elongate over the entire length. A single result of Swiss tests (No. VI) shown in Fig. B differs from the great number of all other

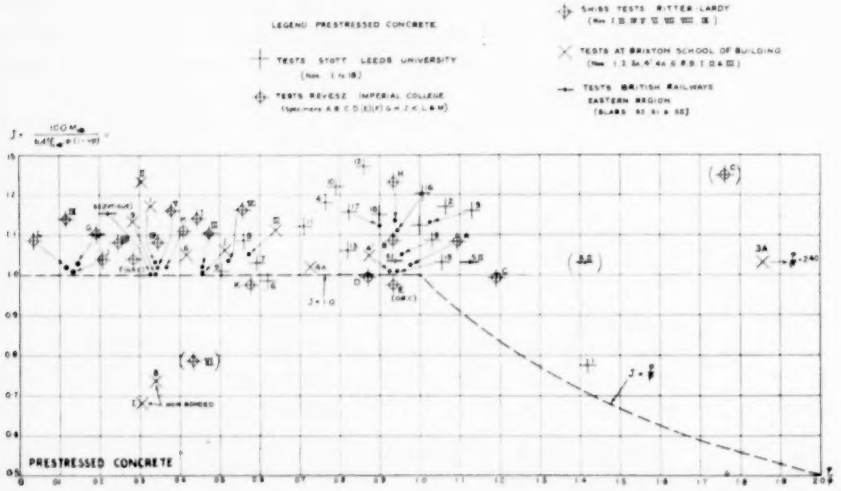

Fig. B—Comparison of actual and computed failure bending moment for various tests (see Fig. 7 of reference 6)

tests and this test result ought to be excluded. There are two tests relating to nonprestressed concrete beams which are denoted by ORC (ordinary reinforced concrete).

In view of the excellent agreement of the practical use of the above safe formulas with numerous test results, it appears of interest to investigate how this would apply to the authors' tests. All specimens in which failure occurred due to shear or bond have been excluded and the investigation has been limited to beams in which the cylinder strength was not less than 4000 psi, since concrete of lower strength was not among the specimens investigated in reference 6.

The computed ultimate bending moments are, except for beam B2, appreciably higher than the actual failure moments, as seen from Table A. Only beam B27 approaches balanced design, while all other beams appear under-reinforced. In these circumstances it is surprising that beams B23 and B24, for which the ratios of percentage $p/\overline{p}$ were as low as 0.571 and 0.602, respectively, are listed in the authors' Table 2 for mode of failure T-C (tension and compression simultaneously). The same applies to beams B18 and B19, for which the respective ratios of percentage are higher (0.758 and 0.674). Beam B7 is marked as having failed in compression only, and in this case the percentage ratio computed is 0.826, which approaches the value 1. In all these beams the prestress was high so that early failure would not have been expected due to reduced prestress in accordance with the authors' second conclusion. This shows that other causes must have affected the mode of failure, *e.g.*, the limited bond resistance of the grout. This may apply to beams B7, B18, B19, and B27, for which the ratio $p/\overline{p}$ is between 2/3 and 1, but it is difficult to understand how the mode of compression failure could have occurred with beams B23 and B24 with respective percentage ratios as low as 0.571 and 0.602. Only beam B2 shows a good agreement between measured failure moment and that computed according to the writer's equations.

There is another point which the writer cannot quite understand. He had assumed that with $k_1 = 1$ and $k_3 = 1$, the optimum possible is obtained, and this is taken into account in his formulas. The factor k_1 governs the shape of the stress block and this cannot be greater than a rectangle with $k_1 = 1$, while the factor $k_3 = 1$ indicates that the maximum stress equals the cylinder strength. However, the authors write that values varying from 0.75 to 1.25 were obtained; and in Fig. 8a a value as high as 1.55 is indicated, though for a concrete of a strength of approximately 1300 psi (apparently beam B26); and for a concrete of strength about 3300 psi a value of approximately 1.15 is plotted for k_1k_3. This would mean that the concrete stress was in excess of the strength. How do the authors explain this?

The writer recently obtained strain measurements at failure of prestressed beams 8 x 12 in. with six pre-tensioned wires 0.276 in. in diameter of a strength of 214,000 psi in the tensile zone. The ultimate bending moments of two such beams were 67.7 and 68.8 kips-ft and after large deformation compressive

TABLE A—TEST RESULTS OF SOME BEAMS AS COMPARED WITH APPROXIMATION FORMULAS

Beam	f'_c, psi	p, percent	$v = \frac{f_{s\,ult}}{200\,f'_c}$	$\bar{p} = \frac{1}{4v}$	$\frac{p^*}{\bar{p}}$	Mode of failure†	Ultimate moment computed, ft-kips‡	$\frac{\text{Measured}}{\text{Computed}}$
B2	5420	0.198	0.227	1.101	0.180	T	21.6	1.02
B7	5910	0.942	0.219	1.140	0.826	C	62.0	0.87
B9	6330	0.418	0.189	1.318	0.317	T	39.7	0.89
B15	5710	0.418	0.210	1.195	0.349	T	40.1	0.89
B17	4580	0.429	0.262	0.955	0.449	T	36.9	0.92
B18	4100	0.647	0.293	0.856	0.758	T-C	43.1	0.89
B19	6220	0.873	0.193	1.295	0.674	T-C	60.4	0.87
B21	6560	0.284	0.188	1.322	0.215	T	27.7	0.92
B22	7630	0.561	0.162	1.538	0.364	T	53.3	0.93
B23	8200	0.943	0.151	1.650	0.571	T-C	68.7	0.85
B24	6120	0.746	0.202	1.241	0.602	T-C	54.0	0.91
B27	4590	0.920	0.270	0.925	0.995	C	60.6	0.85

*The ratio $p/\bar{p}$ represents the ratio of percentage to that of balanced design; for $p/\bar{p} = 1$ the mode of failure ought to be T-C, while for ratios less than 1 the mode should be T.

†Mode of failure taken from Table 2.

‡$M_{ult} = p\,(1 - v\,p)\,\frac{b\,d^2\,f_{s\,ult}}{12 \times 100 \times 1000}$ (M in ft-kips; b and d in in., and $f_{s\,ult}$ in psi).

failure occurred with subsequent fracture of the steel. The writer's formulas of approximation agree well with $k_3 = 1$, while the maximum steel strain ϵ_{su} according to Eq. (5) of the authors' paper obtained from strain measurements amounted to approximately 1 percent, which in turn corresponded to 90 percent of the strength, while in fact the entire strength was reached. In this connection it may be mentioned that in some cases the calculated steel stress may even exceed the tensile strength of the steel; this can be seen from Fig. B for specimens in which $J > 1$. The writer agrees with the authors' assumption No. 3 that no tension is resisted by the concrete. However, due to good bond the necking of the steel may be reduced within the crack, and thus it is possible that in such a case the ultimate force in the steel is greater than that which would occur in the event of ordinary necking. This excess in strength can be taken into account by considering a partial cooperation of the concrete tensile zone.[9]

Concluding, the writer would like to point out that the authors' formulas appear to be in good agreement with their test results and would therefore be of great use in analyzing tests on prestressed beams of similar kind with limited bond resistance of the grout. However, as far as pre-tensioning applies, or in some instances also for post-tensioning with good bond due to perfect grouting, the stress formulas shown by the writer give a much better agreement. The difference may be appreciable, as seen from Table A, and it should be possible to obtain a better agreement than that, even if it is not so close as that shown in Table 2 of the authors' paper. All this obviously relates to bonded steel. The conditions differ further for non-bonded steel. Thus, in fact, three different possibilities must be considered

from which the authors' investigations apparently relate to the intermediate case of bond of limited efficiency.

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By STEPHEN REVESZ*

It is stated by the authors that the investigation ralates to post-tensioned, end-anchored, bonded, prestressed beams. While such a formidable designation of a particular type of prestressed beam may be desirable from the point of view of precision, it is usual to refer to this class of beam as grouted post-tensioned beam. The word bonded is usually associated with fully bonded or pre-tensioned beams. As will be shown later, there is considerable divergence in the performance of pre-tensioned and grouted post-tensioned beams. The efficiency of a grout can not be guaranteed, and the ultimate strength of the high tensile wire is less likely to be utilized than in pre-tensioned beams.

The authors must be congratulated on a valuable investigation concerning the ultimate load performance of grouted post-tensioned beams. The authors assumed that changes in strain in the concrete at the level of the steel are equal to changes in strain in the steel reinforcement. The experimental results confirm that this assumption is justified, and that the steel stress at failure can be accurately predicted from the theoretical formulas in the paper.

The examination of strains in addition to stresses is necessary in post-tensioned beams due to the comparatively high steel strains corresponding to the ultimate strength of the wire. Investigations along these lines were

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commenced by Prof. A.L.L. Baker in 1947 in England,[1] both for grouted and for ungrouted post-tensioned beams. It was found that the ratio between the steel strain change and the "virtual" concrete strain adjacent to the steel was 1.0 for grouted beams, and as low as 0.1 for ungrouted post-tensioned beams. Dr. Hajnal-Kónyi[2] showed that in fully bonded beams the value of 1.0 could be exceeded.

The authors established an empirical relationship between k_1k_3 and f_c' based on the neutral axis depth at failure. If, however, k_1k_3 is expressed as a function of f_c' in the formulas, it is surprising to find that a constant coefficient is employed for k_2, since this must be also affected by the plasticity or brittleness of the concrete. The important consideration is the final effect on the lever arm ratio. It will be found that any variability of k_1k_3 and k_2 with f_c' is self-compensating to a large extent, and therefore both coefficients could be taken as constants (such as in Whitney's expression) without great error in the lever arm ratio.

Comparing the result of tests of pre-tensioned beams with the authors' test beams affords some interesting observations. In Fig. C the results of tests of pre-tensioned beams from reference 3 have been plotted with M_u/bd^2f_c' as ordinate against pf_{su}/f_c' as abscissa. (The concrete strength was measured on 6-in. cubes and the results have been adjusted by 0.85, as the relationship to cylinder strength.) The beams were reinforced with 0.105-in. diameter wire of 296,000 psi ultimate strength. With the exception of beam E, which failed by excessive elongation, the other beams failed by fracture of the reinforcement, although adequate warning of failure was given by deflection and wide cracks. The ratio of prestress to ultimate strength was as follows: (1) beams F and E—0; (2) beams G and H—0.35; and (3) beams A, B, K, M, L—0.6.

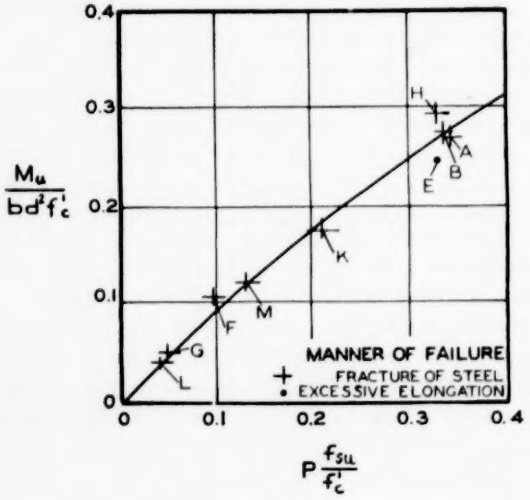

Fig. C—Results of tests on pre-tensioned beams according to authors' definition

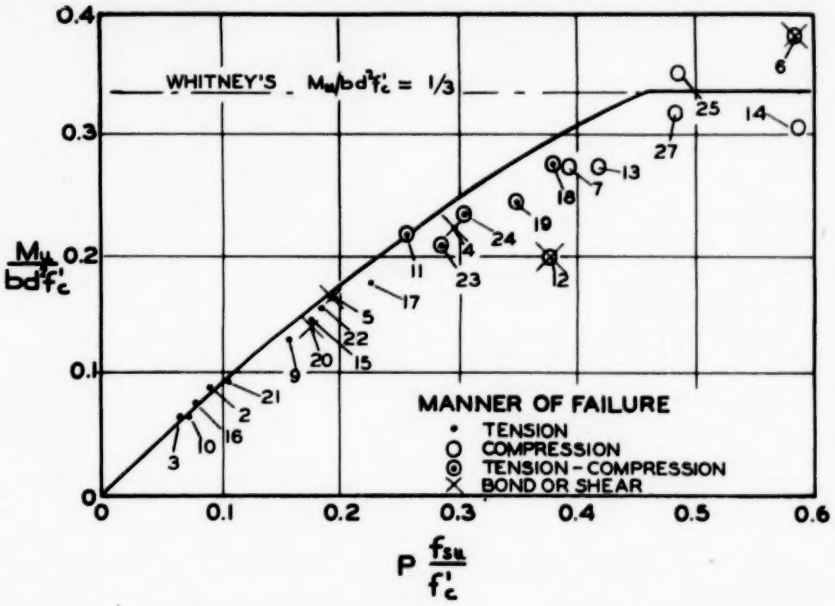

Fig. D—Authors' test results plotted on same scales as Fig. C

The experimental results confirmed that in pre-tensioned beams, where the bond is good, the ultimate strength of the steel as high as 296,000 psi can be utilized, even with moderately low prestress. Indication is that the local steel strain at a crack might be greater than the virtual strain in the adjacent concrete fiber.

In Fig. D the authors' test results are plotted on the same ordinate and abscissa as Fig. C. Beam B26 is not shown as the concrete strength was well below the value which would be permitted in practice. In plotting pf_{su}/f_c', the ultimate strength of the steel was used. The diagram serves to illustrate how far short grouted beams would fall below expectations if treated in the same manner as pre-tensioned beams. The tests were made soon after grouting. In spite of the good results on the control cylinders, it is probable that better results could have been obtained if the grout had been allowed to mature a little more. Of the 27 beams tested, five were reported to have failed in shear or bond. It is not easy to differentiate between bond and shear failure since a diagonal tension crack may appear due to local bond failure in a prestressed beam. In the writer's opinion, the five premature failures may have been avoided by allowing the grout to attain a greater strength in the beams.

It is noticeable that values falling above $M_u/bd^2f_c' = 0.2$ have been referred to as tension-compression failures, and above $M_u/bd^2f_c' =$ about 0.27 as compression failures. By the authors' definition, compression failures denote over-reinforced beams. It is stated by the authors that "Actually, as far as

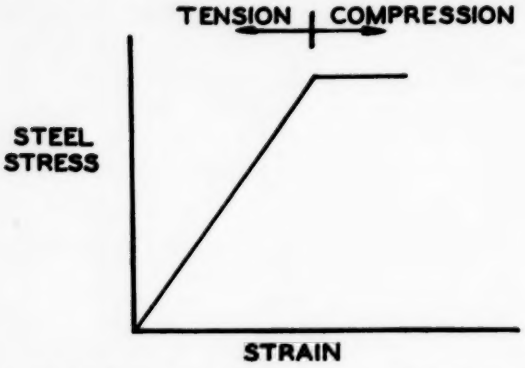

Fig. E—Diagram showing "tension" failures

the behavior and strength are concerned, there is no sharp transition between under- and over-reinforced beams." This is not surprising, since the authors have given the words a meaning which is not customarily employed. Beams which failed while the steel stress was below the "yield point" have been designated as tension failures. This is illustrated in Fig. E. This definition is incorrect and is based on a misconception. The terms under- and over-reinforced apply to the concrete section under consideration, and not the particular type of steel employed. The true meaning of over-reinforced beam is that the ultimate bending moment resistance of the section can not be appreciably increased by more reinforcement or additional prestress. In the authors' test below the value of $M/bd^2f_c' = 1/3$, this was not the case, and the beams were under-reinforced despite the fact that the yield point of the steel was not reached. This is shown in Fig. F, which is normally used in the analysis of normal reinforced concrete beams. When the limit is reached, failure has been found to be explosive, without the warning given by wide cracking. For this reason, primary compression failures are to be avoided in practice, and in the writer's opinion, the limit should be below the value of 1/3 for reasons expounded elsewhere.[3]

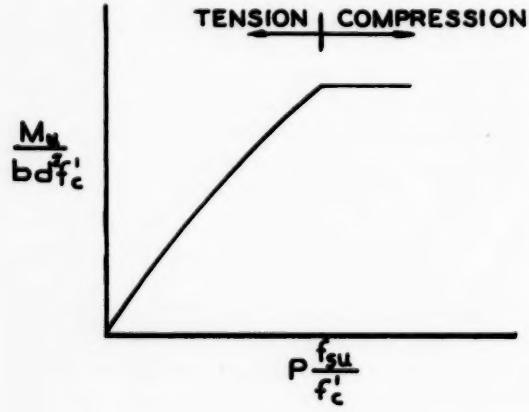

Fig. F—Normal diagram for analysis of ordinary reinforced concrete beams

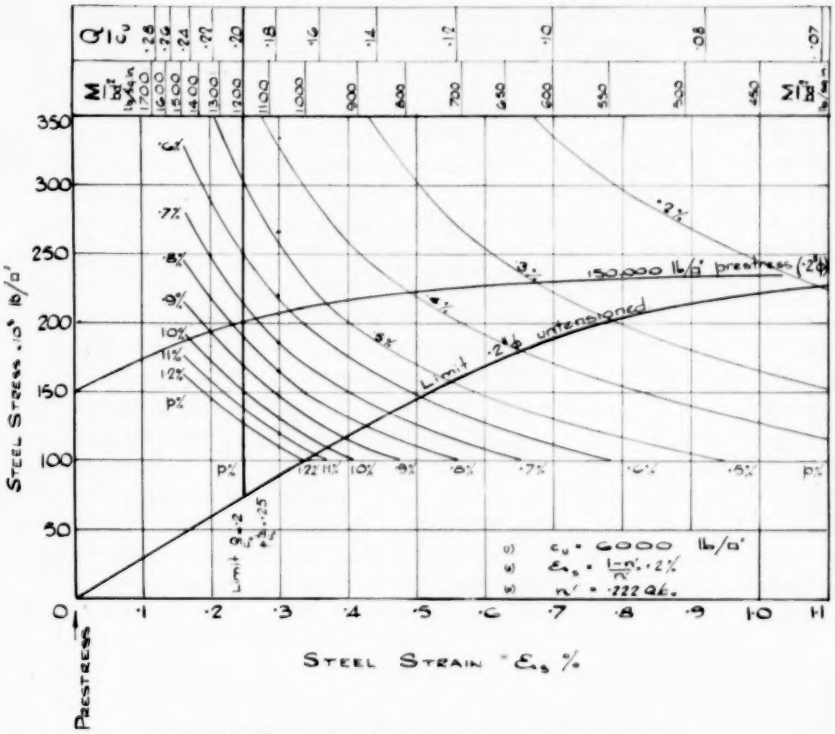

Fig. G—Design chart for grouted post-tensioned beams

The problem of defining under- or over-reinforced prestressed beams raises an important issue. If it is not possible to reach even the yield point in an under-reinforced post-tensioned grouted beam, with a certain degree of prestressing, is the particular type of steel economically utilized in the beam? The ultimate strength or the yield point of a high tensile steel should be carefully chosen for a particular case of prestressed beam, and the prestress of the steel should be related to the stress-strain characteristic of the steel.

A chart may be drawn up such as Fig. G (reproduced from reference 3) for grouted post-tensioned beams, where the curves represent various steel percentages. A more general one with p/f_c' could also be used. The stress-strain diagram of the steel can be drawn on tracing paper and superimposed on this chart in such a way that the prestress on the steel diagram intersects with the LHS ordinate of the chart.

The maximum steel stress can thus be found, corresponding to a particular value of p/f_c', and it now remains only to choose a steel of a certain stress-strain characteristic to utilize it fully with a given prestress.

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AUTHORS' CLOSURE

Both discussers have shown great concern over the effectiveness of the grout in bonding the steel reinforcement. This item was also of concern to the authors during their tests. However, the only apparent bond failures were in beams B12 and B20. During the tests, frequent observations were made of the load on the dynamometers at the ends of the beams. In only one case, that of B20, was an increase in stress observed, denoting bond failure to the end of the beam. In addition, comparisons were made between strains measured on the surface of the beams and on the reinforcement. These two strains agreed closely indicating that there was little if any slip between the steel and concrete as would be the case in extensive bond failures.

The primary concern of both Mr. Revesz and Dr. Abeles with regard to bond seems to stem from the fact that a large percentage of their beams failed by fracture of the steel while none of the beams tested by the authors failed primarily in this manner. Dr. Abeles mentioned a beam which failed first by crushing and second by fracture of the reinforcement. Several of the beams tested by the authors failed in a manner similar to this, but only after loading was continued beyond the point where the concrete had crushed at the top of the beam. It is difficult to see how fracture of the steel could precede crushing of the concrete with steels having elongations as large as those used in the beams tested by the authors, except possibly when the beams are loaded with one concentrated load, causing a concentration of strain below the load. Could a difference in manner of loading or in steel elongation account for the difference in type of failure?

In Fig. D of Mr. Revesz's discussion, the authors' beams are plotted in the same manner as is done for Mr. Revesz's beams in Fig. C to "illustrate how far short grouted beams would fall below expectations." If the two figures are compared closely, it can be seen that all tension failures fall close to the same trend line as Mr. Revesz's beams, tending to indicate that the authors' beams do not fall much below expectations, except for compression failures which should fall below the trend line for tension failures. Virtually the same is true for the comparisons made by Dr. Abeles in Table A. All of Dr. Abeles' comparisons were made for beams with rather large percentages of steel except for B2 and B21. Consequently, the steel stress at failure of the beam should be somewhat below the tensile strength of the steel. Dr. Abeles' approximate formula gives an upper limit for the moment capacity and should give good results for beams with normal percentages of steel.

Both discussers advocate using a constant value for k_2/k_1k_3 in computing ultimate moments. This procedure will give good results as long as the steel stress at failure is near some constant value—such as the yield point in ordinary reinforced concrete, or the tensile strength in prestressed concrete as would be the case in under-reinforced beams. However, the use of a constant value for k_2/k_1k_3 does not give good results for over-reinforced beams and renders it necessary to establish a maximum value for $M/f_c'bd^2$. While this procedure probably is sufficiently correct for design purposes, it does not give a picture of the deformations or of the conditions of the beam at failure. This is true of any procedure which does not take account of both the *strain* and *stress* conditions.

Dr. Abeles expressed concern that the concrete stress was greater than the concrete strength. This of course is not possible. However, it is possible and probable that the strength of concrete in a beam is different from the strength of a *cylinder* of the same concrete just as the strength of a *cube* of concrete would be different from a *cylinder* of the same concrete. A large number of factors are possibly involved in the value of k_3, but since the authors' beams were all of the same size, k_3 was plotted using concrete strength as the only variable.

There is definitely a difference of opinion between the authors and Messrs. Revesz and Abeles as to the meaning of the terms under-reinforced beams and over-reinforced beams, and tension failures and compression failures. Any definition of these terms must refer to an arbitrary dividing point. The authors felt that the definition based on the steel stress at failure corresponded more closely than any other to the definition for ordinary reinforced concrete beams based on whether the steel stress at failure was above or below the yield point. This definition not only refers to steel stress and mode of failure, but also refers to deformation at failure as illustrated in Fig. 9. Thus, by the authors' definition, the dividing line between under- and over-reinforced beams defines the steel stress at failure, and the deformation conditions, that is, the point at which there is no reserve deformation between yield and failure. The authors feel that over-reinforced beams should be avoided because of their "brittleness;" thus to avoid a "brittle" beam by avoiding over-reinforced beams, the definition must be tied to deformations.

Mr. Revesz seems to have misunderstood the authors' definition of tension failures, for beams which failed while the steel stress was below the yield strength were designated as compression failures, not as tension failures as indicated by Mr. Revesz in Fig. E of his discussion.

Mr. Revesz also takes issue with the designation of the beams tested. The designation, post-tensioned, end-anchored, bonded, prestressed beam, was made in accordance with the proposed definitions of ACI-ASCE Committee 323.*

Dr. Abeles has made a careful study of the cracking loads of the beam tested. None of the authors' beams developed computed tensile stresses in

*"Proposed Definitions and Notations for Prestressed Concrete," ACI JOURNAL, Oct. 1952, *Proc.* V. 49, p. 85.

the concrete before visible cracking as large as those reported by Dr. Abeles. There was a large scatter in the modulli of rupture, but no larger than is usually found in such tests involving a large number of different concrete mixes tested at ages ranging from 29 to 100 days. Thus it appears that one explanation for the lower tensile stresses at cracking would be lower tensile strengths for the concrete used in the authors' beams.

The authors wish to thank Messrs. Revesz and Abeles for their discussions. They are pleased that the paper has elicited such close study and thought-provoking comments, especially with regard to the efficiency of grouting and bond. The authors also wish to state that the tests reported are preliminary tests and the future program of the investigation includes studies of pre-tensioned beams and, if possible, beams with imperfect grout conditions.

Discussion of a paper by J. John Brouk:

Perlite Insulating Concrete*

By J. O. CHERTKOF and AUTHOR

By J. O. CHERTKOF†

For years perlite has been expanded and used as a lightweight aggregate but perhaps the greatest phenomenon of perlite has been treated too lightly. Because of the economic uses of expanded perlite as a lightweight aggregate, industry did not explore the most important factor—adsorption—and did not endeavor to make use of this characteristic. It has been known that when expanded perlite was employed as a lightweight aggregate in some particular uses, depending on the character of the finished product, the resulting mass had been found to have considerable so-called "suction." In some cases, expanders endeavored to overcome this suction without giving much thought to the cause and the best control of this characteristic.

It will be attempted in this discussion to explain in brevity some of the findings on expanded perlite, the factors that control or vary the phenomenon of adsorption, and a method of solving and controlling this phenomenon in lightweight insulating concrete.

For simplicity, expanded perlite throughout this paper will be termed "aggregate" and perlite ore will be considered as "ore." Since aggregates are made through the process of expansion by heat and the aggregate densities are controlled by various expanders from "under-" through "over-expansion," the resulting adsorption of a finished aggregate will vary proportionately with the type of ore, the method of expansion, the density and shape of the aggregate, and the percentage of the various fraction sizes in the packaged aggregate as sold by the expander. If each variable is considered separately and the others kept constant, the explanation of what takes place by changing one variable can be understood more easily.

Let us first consider what happens when the source of ore is changed and the rest of the variables remain constant. We will assume that only the ore deposit is changed and that the presized shipment to the expander is the same regarding the percentage of varying fractions. The expander will find that he can develop his technique of handling the new ore, and the only difference will be that the melting point of the new ore may change his operating temperature ± 150 F. The lower temperature ores naturally will require

*ACI Journal, June 1954, *Proc.* V. 50, p. 857. Disc. 50-50 is a part of copyrighted Journal of the American Concrete Institute, V. 26, No. 4, Dec. 1954, Part 2, *Proceedings* V. 50.

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less fuel for processing, and control of expansion techniques will enable the expander to control the aggregate fractions within reasonable limits. The converse is also true; that is, if the new ore has a higher melting point, it will require more fuel or a longer heating period before expansion takes place, which increases the fuel consumption.

One might assume that the lower temperature ores would have a thinner film enclosing the air cells making up a particle of aggregate in comparison with the higher temperature ores, and therefore that the lower temperature ores would be more friable. This, however, is not true. If it is considered that one particle of the ore is expanded eight times its original size and a second ore particle, of the same identical size but of a different ore, is expanded eight times its size, then the amount of mineral and the number of air cells in the aggregate has to be the same, as the densities remained the same. Numerous people have been questioned to clarify the opinion of what happens when the ore expands and in many instances it has been explained that the particle of ore expands in an irregular enclosure or bubble. This, it is believed, is not true.

It might be well to use a simple analogy to illustrate the findings of a microscopic examination of a particle of aggregate. If we would consider a particle of ore as being the shape of a pear before same has been expanded and the aggregate particle after expansion as being a cluster of grapes, we get close to the actual picture. The ore when expanded shatters completely and, actually, the bubbles or individual grapes which were formed have broken; but, because the entire pear was molten and the inherent moisture or gas in the ore exploded same, it is more plausible to assume that the larger portion of the grapes in the cluster have broken completely and, in turn, sealed the spaces between each individual grape. The resultant aggregate particle then has all the exterior grapes broken and these, in turn, have fused with the adjoining broken grapes and sealed the spaces between same. The number of projecting pieces or burrs left on the cluster depends on the technique of expansion. If the aggregate can be held in the furnace after it has been expanded so that the burrs or points will melt or fuse with the adjoining surfaces, then the aggregate particle is no larger when one ore or the second ore is used. There should be no appreciable difference in hardness of the finished product if the expander develops and perfects his technique. It is believed that hardness does not influence adsorption, but the surface area of the particle does.

The next variable to be considered is the shape of the aggregate particle. This definitely has some influence on adsorption. The shape of the aggregate will vary with the ore supply, because the entrapped moisture or gas is not the same in all ores. When the entrapped moisture is greater in one ore in comparison with a second, the explosive force is greater. However, controlled expansion technique can produce a similar finished aggregate particle of the same size, as explained above. This also influences the density of the aggregate.

It has been recently acknowledged that lightweight perlite aggregates are not being offered on the market for comparison of friability in itself, and industry now is comparing the various aggregates by making hydraulic mixtures and determining yield. It has been proved that if any ore is processed properly the difference in yield will be ± 3 percent when tested in a hydraulic mixture, and this can be attributed to ore supply, presizing, fraction percentage, and adsorption if techniques are the same. The burden of responsibility regarding the proportioning of fractions is that of the individual expander, as he can over-expand or under-expand any presized ore and, depending on the equipment which he is using, one particular ore may lend itself better to his operation than another. But he should always recognize that he must have a sufficient percentage of each particle fraction in his resultant aggregate so as to fill up most of the voids between the largest particles present. This will then permit the binder to be used as a binder, instead of a bulking material. One of the great troubles found is that the percentage of fractions is not controlled sufficiently, which causes the consumer to complain that he has a poor mix and has to use more binder.

The most important thing, however, is the control of adsorption. It must be remembered that hydraulic binders are of very fine mesh, minus 200 mesh or finer, so as to permit rapid hydration, and the aggregate itself has voids which are large enough to permit this extremely fine binder to actually enter the aggregate particle. The force which draws this binder into the aggregate void is known as surface tension or adsorption, both being one and the same. This force can hold 75 percent of the liquid and/or the binder permanently, or until the aggregate particle is broken, unless some means is taken to decrease this force.

It is acknowledged that a tremendous number of these aggregate voids are microscopic and irregular. However, the total surface area with which we are dealing in 1 cu ft of aggregate is enormous. In many cases, the liquid and binder of a hydraulic mixture bridge these voids and this reduces the amount of surface area, effecting adsorption. It has been determined that perlite aggregate adsorbs 300 to 500 percent of its weight in water. *This is the important phenomenon of perlite, the fact that perlite aggregate is one of the best adsorbents for liquids and gases known today, based on the weight of the aggregate.*

The many advantages of this phenomenon cannot be presented at this time; however, the control of this force of adsorption in perlite aggregate when used in a hydraulic mixture of water and portland cement as a binder will be explained.

Since it has been explained and proved that perlite has a great force of adsorption and that the water and portland cement will enter the voids, the problem is one of how to prevent this from taking place. It can be done by completely sealing the particle of aggregate, which would increase the cost and density of the aggregate and some of the advantages would be lost when used in lightweight concrete. A solution may be found at some time

where this would be advantageous and economical. For the present, a solution to this problem has been found which is economical and satisfactory. That is to coat the aggregate with a gaseous inhibitor, which inhibitor acts and performs as a wetting agent and reduces the surface tension; and secondly it expands when used in a mixture with water and portland cement, which results in sealing the voids so that the binder does not remain in these voids. It is acknowledged that the smallest particle size of any liquid or solid can be obtained when said liquid or solid is changed into the gaseous state. Therefore, if the coating is performed with a gas, every surface surrounding the voids is completely coated, regardless of its microscopic size. This coating can be applied during the course of manufacture or expansion of the ore, as the ore reaches temperatures in excess of 2000 F and is cooled to temperatures as low as 150 F. With this range of temperatures, depending on the inhibitor being used, the proper point of introducing the gas can be determined.

After the aggregate has been coated, it is ready to be used in a hydraulic mixture of water and portland cement. If a slurry is made with the water and cement and then the aggregate is added, it has been found that the mixture performs in the following manner:

(1) It will be noted that the aggregate adsorbs all of the slurry and the entire mass appears dry.

(2) After a short time—perhaps 2 to 5 min, depending on the type of mixer and speed of mixing—the mixture becomes wet, which can only take place if the aggregate is destroyed or broken and/or the slurry is forced out of the void. It will be found that with a coated aggregate, as described above, the slurry of water and cement is actually forced out of the void.

(3) If the density is checked immediately after the short period of mixing, it will be found that the density of the mixture is lightest at this point.

(4) As additional mixing takes place, the density will start to increase and will reach a point of consistency after which no further change will take place, regardless of mixing time, within reasonable limits.

The reason that a lighter density is obtained at first and then the densities increase is due to the same function of adsorption, that is, that after the coating on the surface of the void has expanded, it expands a little further than the edges of the void and then breaks. When the bubble has broken, the liquid and binder are then attracted back to the edges of the void in the aggregate particle and a greater density is obtained. The portland cement (binder) can be prevented from entering this void by having extremely fine particles of aggregate in the mix. These fine particles should be as fine as, or finer than, a minus 200-mesh screen and this fine aggregate then acts as a filter through which only the water of the slurry can penetrate.

It will be found, also, that an exacting amount of coating will influence the initial wetting time, density, and water necessary for the hydraulic mixture. An excessive amount of coating will reduce the wetting time because, as previously stated, the inhibitor is basically a wetting agent. Vigorous mixing when excessive coating is present on aggregate will cause air to be entrapped. This would weaken the ultimate strength of the concrete if the

entrapped air would remain. However, since the adsorption has been controlled and not destroyed, the particles compact with the binder to obtain the predetermined density, as previously stated. The perlite industry is interested in selling aggregate and not air. Entrained air reduces the setting time of the concrete mix and, therefore, the fractions should be of such size as to fill all voids and the binder will, in turn, coat each particle and thus give the strongest ultimate concrete results. Since the aggregate is initially light in density, 10 or 15 percent additional aggregate in relation to the amount of portland cement used will not increase the densities over 1 or 2 lb per cu ft of finished concrete. Yet it will be found that the strengths will be increased 100 to 200 percent if the air is eliminated and aggregate takes its place.

An excess amount of water in the mixture does not necessarily reduce the strength of the concrete, but actually helps to make a denser mass which, as explained above, eliminates entrapped air and affords a stronger concrete. This excessive water leaves the portland cement by entering the voids. It has been found and proved that the excess water which has entered the voids of the aggregate will actually be beneficial to the curing of the concrete, as this water supply is called upon during hydration of the cement.

Lightweight insulating concrete has been placed in areas where the temperatures have been as high as 300 F and no ill effect or reduction of strength was noted. Ordinary concrete would have cracked, broken, and set within 15 min, yet the concrete made with perlite was still moist, not cracked, and had not reached its final set after 3 hr under these conditions. It is predicted that every cubic yard of ordinary concrete will eventually have a certain percentage of perlite, to obtain this ideal curing condition, which will not adversely affect the strength but actually increase same and reduce cost of curing.

It is advisable to state at this time what happens when less water is used in the hydraulic mixture with the coated aggregate and the extremely fine particles as a binder. It has been found that the coating acts in the same fashion. However, the bubble which leaves the void and appears in the mixture between the aggregate particles remains and, therefore, the density is reduced. This gives a weaker concrete. The reason that this bubble is retained in the mixture is due to the fact that the slurry consistency initially was too thick and the bubble of air was trapped and could not be broken when the filtering process took place. In some instances, where extremely light and weak concrete is required, this might be an advantage.

It is felt that concretes made with perlite aggregate should be classified in three separate and distinct fields of use: (1) lightweight insulating concrete, with strengths of 150 to 300 psi, where the insulation is the most important factor and the strength is secondary; (2) lightweight insulating structural concrete, with strengths of 200 to 600 psi, where the strength and insulation are factors of equal importance; and (3) lightweight structural concrete, with strengths of 1000 to 2000 psi, where strengths and densities are the most important factors and insulation has little or no value.

AUTHOR'S CLOSURE

Discussion offered by Mr. Chertkof is extremely interesting and he is to be congratulated for the valuable research on the subject.

From Mr. Chertkof's investigation, he states that expanders, if they perfect their expanding technique, can make a finished product that has no appreciable difference in hardness regardless of the source of ore used. This is entirely possible, but many expanders have not as yet learned to manufacture their product perfectly. It is entirely true that there are many lower temperature ores that do produce very hard and nonfriable particles.

It is also true that under microscopic examination you will find particles expanded similar to a cluster of grapes, but with some aggregates you will find complete irregularity at the surface—sharp irregular edges protruding which have been termed by some in the industry as "rabbit ears." These "rabbit ears" and protrusions from the particles tend to break off in the mixing process, resulting in a much finer gradation and a decided loss in yield.

Regarding gradation, the theory of having each particle fraction in a resultant aggregate fill up most of the voids between the larger particles present has not been completely proved to be the ideal way to sell an extremely lightweight aggregate. Exceptionally high strength to weight ratios have been developed in perlite concrete using absolutely no fines whatsoever.

Regarding the question of adsorption, perlite aggregate can be expanded to adsorb a minimum amount of water into the interior of each particle and, as Mr. Chertkof ably suggests, can also be controlled by the use of a gaseous inhibitor. This gaseous inhibitor can be added in many forms: by simple air entrainment or by, as he said, precoating of the particles, but this should be done in such a way that the total water content for the concrete should not contain more than 54 gal. of water per cu yd.

Mr. Chertkof states that vigorous mixing results in having too much air entrapped and that the entrapped air would weaken the concrete. There are many cellular concretes in the market that make use of no aggregates whatsoever. The strengths are comparable to other concretes using lightweight aggregates.

Mr. Chertkof states that the perlite industry is interested in selling aggregate and not air. However, the buyer of the aggregate is interested in developing a satisfactory lightweight concrete at a minimum cost to himself. He also states that the excessive amount of water does not necessarily reduce the strength of concrete. In our investigation, this excess water gradually leaves the concrete, leaving in its place possible voids. The amount of water remaining to each bag of hydrated cement is approximately 20 lb per bag of cement.

I believe Mr. Chertkof is very optimistic when he states that every cubic yard of concrete will eventually have a certain percentage of perlite. For certain conditions, perlite additive has been beneficial; for others, results are not too satisfactory.

Discussion of a paper by Phil M. Ferguson, Robert D. Turpin, and J. Neils Thompson:

Minimum Bar Spacing as a Function of Bond and Shear Strength*

By K. HAJNAL-KÓNYI, C. A. WILLSON, and AUTHORS

By K. HAJNAL-KÓNYI†

The tendency of deformed bars to split the surrounding concrete is an inevitable consequence of their improved bond. Full advantage of high bond strength can only be taken if failure by splitting is safely avoided by an appropriate arrangement of the reinforcement. The paper shows that the cover to the bars is of hardly any influence and that the most important factors are the provision of stirrups and the spacing of the bars.

It is rather surprising to European engineers that in the United States beams without stirrups are used. Both on the Continent and in Great Britain it is standard practice to provide stirrups in beams irrespective of the magnitude of the shear stresses (*e.g.*, according to the German code it is compulsory to use stirrups in beams).

While in the pull-out tests some specimens were made with and some without stirrups, in the beam tests apparently all specimens were left without stirrups from the point of view of bond failure. As stated in the paper, "extra web reinforcement *above* the bars was used in a number of cases to force a bond failure where diagonal tension weakness was suspected." It is obvious that these stirrups had no influence whatever on the prevention of splitting of the concrete surrounding the longitudinal reinforcement (see Fig. 11 and 23). Thus none of the beam tests is applicable to what European engineers would consider practical cases. It can hardly be doubted that the result of beam D4 illustrated in Fig. 23 would have been quite different if stirrups had been provided at the lap.

The writer recently made a number of tests on beams with cold worked deformed bars.‡ Of these, the results of two pairs of beams are of interest in connection with the present paper. Fig. A shows the cross section and method of loading of beams B3, B4, L1, and L2. The quantity of stirrups was governed by the requirement that failure by diagonal tension should be avoided. This has been achieved since all four beams have failed by practically reaching the ultimate strength of the steel and, in fact, in beams L1 and L2 the

*ACI JOURNAL, June 1954, *Proc.* V. 50, p. 869. Disc. 50-51 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 26, No. 4, Dec. 1954, Part 2, *Proceedings* V. 50.

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‡Hajnal-Kónyi, K., "Recent Research on Deformed Reinforcing Bars," *The Reinforced Concrete Review* (London), V. 3.

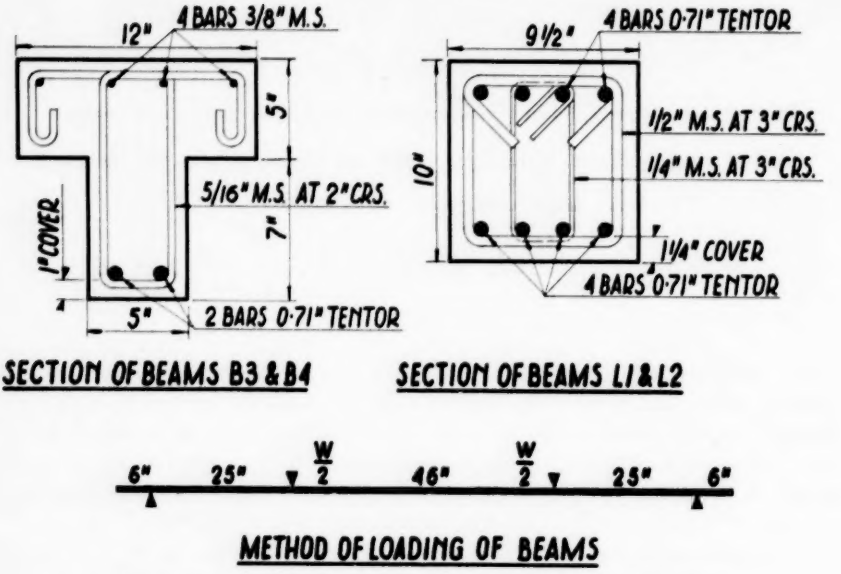

Fig. A—Cross sections of test beams and method of loading of beams

bars fractured when the test was continued beyond the maximum load. The relevant data and test results are summarized in Table A.

All bars protruded beyond the ends of the beams and were controlled by dial gages. None of the bars showed any sign of slipping. Slight longitudinal cracks, indicating a tendency toward bursting, appeared in the central ranges of the beams where theoretically no bond stresses occur and where the bars were stressed to their ultimate strength. They did not appear in the ranges of maximum bond stresses where stirrups at close spacings were provided.

It should be noted that the concrete strength was much lower throughout than in the tests described in the paper. Converted into cylinder strength, f_c' in beam B4 was below 2000 psi, *i.e.*, less than half of the average reported

TABLE A—SUMMARY OF TEST RESULTS

	B3	B4	L1	L2
1. Beam mark	0.65	0.65	2.05	2.00
2. Tensile reinforcement, percentage*	2.52	2.52	2.34	2.34
3. Effective bar spacing†	43.6	43.6	43.6	43.6
4. Embedded length to loading points†	3640	2390	3080	3270
5. Average cube strength, psi	96,880	95,540	91,340	91,500
6. Average ultimate strength of steel, psi‡	56	56	83	85
7. Maximum sustained load, kips	95,480	95,480	92,800	94,100
8. Steel stress at failure, psi§	620	620	643	651
9. Maximum shear stress on concrete, psi	687	687	686	694
10. Maximum local bond stress, psi**	2.89	2.89	2.81	2.85
11. Factor of safety for f_s = 33,000 psi	238	238	244	243
12. Bond stress at working load, psi				

*Based on actual dimensions.
†In terms of bar diameter D.
‡Each bar was tested.
§Calculated by the standard method, $n = 15$.

**Calculated by $u = \frac{V}{\Sigma o\, jd}$

in the paper. Nevertheless, the bond stresses reached in the beams compare well with the values for "heavy stirrups" in Fig. 15. It should be borne in mind, however, that the figures in line 10 in Table A do not represent bond strengths since none of the bars slipped at the stresses reached. These might have been exceeded if the tensile strength of the bars had been greater or the distance between support and loading point shorter.

In the writer's opinion, structures should be so designed that if they are loaded to collapse, failure by bond, diagonal tension, or compression (except, of course, for compression members) is avoided under any circumstances. The latter mode of failure is automatically excluded by adherence to the present codes in all countries, but to exclude the possibility of failure by bond and diagonal tension, more severe specifications regarding stirrups and bar spacings are necessary than those in the present ACI Code and in several of the European codes.

More research is needed for the establishment of safe limiting values. It is probable that the diameter of the bars is also of great influence and conditions become more critical with increasing diameter. The result of beam C20, of the paper, is an indication in this direction. It would be desirable to carry out tests on beams with the maximum size bars available, but tests on beams with deformed bars without stirrups would be a waste of effort since it is a foregone conclusion that their results cannot be satisfactory.

The writer definitely disagrees with the suggestion in item 5 of the conclusions that "a proper allowable bond stress for splices (*in the absence of stirrups*)* . . . is near to 225 psi for $f_c' = 4000$ psi." *No splices of deformed bars should be permissible without an adequate quantity of stirrups.*

The same applies to all parts of a structure enumerated in item 4 of the conclusions. The remedy should be the provision of stirrups and a more severe limitation of bar spacings to prevent splitting of the concrete rather than a reduction of the bond stresses which, as the paper seems to indicate, are not excessive *if* splitting of the concrete cannot occur before the full stress in the steel (yield point or ultimate strength according to the quality of the bars) is developed.

Submitted for the Committee on Reinforced Concrete Research of American Iron and Steel Institute by C. A. WILLSON†

This paper seems to be a condensed version of a paper, "Minimum Spacing of Bars in Precast Elements, Part II," presented by the same authors in August, 1953, as a bulletin of the University of Texas. If this is a study of bar spacings in precast elements, there should be some reference to "Minimum Standard Requirements for Precast Concrete Floor Units (ACI 711-53)"[1] and to the paper "Spacing of Moment Bars in Precast Joists," by Menefee and Kinnier.[2] However, the later condensed version attempts to cover the much broader subject of bond in general, but there is no reference to the extensive literature on bond.

*Italics are the writer's.

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In this report on a research program, a great deal of space has been devoted to the presentation of arguments against the further use of the allowable bond stresses specified in the current ACI Building Code. These arguments are based on the low values obtained from tests on unusual specimens. Comparisons with results of other investigations lead to serious questions as to the validity of some of the test data. In the following discussion, attention is called to certain factors which may account for the low values obtained.

As stated in the paper, this program covered the testing of 230 pull-out specimens and 42 beams. Such a large number of specimens would indicate that a broad range of variables was involved, but unfortunately, this was not the case. Since no tabulations of test data were included in the paper, reference was made to the University of Texas bulletin. Information secured from Appendices II, III, and IV on pp. 70 to 79 may be summarized as follows: (a) All 272 test specimens were made with high early strength cement and with aggregate having a maximum size of ¼ in., (b) 252 were made with one mix of concrete, (c) 249 were tested at the same age (8 days), and (d) 192 were made with the same bar size. Furthermore, out of the 208 eccentric pull-out specimens tested, (e) 125 failed at bond stresses under $0.10f_c'$, (f) 101 failed at bond stresses under $0.08f_c'$, and (g) 39 failed at bond stresses under $0.05f_c'$. (h) In addition, out of the 42 beams, 15 specimens (three with stirrups) failed at bond stresses under $0.08f_c'$. Since bond stresses at failure normally vary from $0.20f_c'$ to $0.30f_c'$, these values are quite startling.

Throughout this program the test specimens were designed in such a way as to cause unnatural rotations under load applications, thus creating progressive localized excess strains which resulted in an abnormal splitting action. While such an action must have been planned intentionally, it does not simulate the circumstances encountered in practice. In addition, most of the specimens were tested in a damp condition which reduces bond strength as compared to dry specimens.[3] Furthermore, most specimens were cast with bars in a vertical position and pulled in the same direction as the concrete was cast. This gives low bond strength as compared to bars as cast in a beam.[4] Explanatory notes accompanying these tabulations show that in eight cases (four pull-outs and four beams) initial shrinkage cracks were observed before testing began. With such a large proportion of the test specimens failing at abnormally low values of bond, undiscovered shrinkage cracks might have led to many of the other premature failures.

The dimensions of the various test specimens have been examined to see how they comply with the requirements for "effective bar spacing" as well as the standard specifications of the ACI Building Code for minimum spacing and cover. A study of the data shows that only 70 of the 208 eccentric pull-outs and only 12 of the 42 beams complied simultaneously with the two sets of requirements. In this paper the authors question the suitability of beam tests for the study of bond, and therefore, as stated on p. 872: "The eccentric pull-out test was devised to give a simple test that would simulate the worst beam conditions."

When the Committee on Reinforced Concrete Research of American Iron and Steel Institute was faced with the problem of devising test methods which might be used for the comparison of the bond effectiveness of different types of deformed reinforcing bars, the assistance of the ACI Committee 208, Bond Stress, was enlisted. As a result the well-known beam test procedure[5] was developed. Tests of this type were selected because they simulate the actual conditions of practice and therefore are suitable for the establishment of building code requirements. Such design standards cannot be developed from the results of tests on abnormal or unusual specimens. This beam test procedure was followed in great detail in all of the investigations of bond carried out at the National Bureau of Standards by Arthur P. Clark, under the Research Fellowship of the Committee on Reinforced Concrete Research of AISI, as reported in his paper.[6] During 1947-48, on four different occasions, progress reports on the results of these tests were presented to the members of a special subcommittee of ACI Committee 208 for consideration. Additional test data bearing on the same subject were presented by C. A. Menzel. While these investigations were being carried out, improvements in the design and production of deformed concrete reinforcing bars were being made to meet the increasingly rigid requirements for deformed bars which were being developed by the concrete reinforcement subcommittee of Committee A-1 on Steel of the American Society for Testing Materials.

When all of Clark's bond tests were completed, the final results were presented officially to the full membership of ACI Committee 208 for consideration. After approval was secured from that group, similar presentations were made to the appropriate subcommittee of ACI Committee 318, Standard Building Code, and the full membership of that committee. In due course, approval was granted by all of these groups culminating in the official revision of the ACI Building Code in 1951. As a net result, for the first time in history there was available to the users of concrete reinforcing bars a minimum standard specification for the deformations of these bars and a set of bond stress values which would apply only to bars which met those specifications.

In conclusion: (A) this test program is insufficient in breadth of scope or range of variables to justify general statements of conclusions; and (B) the abnormal conditions set up in these tests are not suitable for the development or revision of standards of design practice.

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AUTHORS' CLOSURE

The discussions of this paper have related entirely to bond strength and splitting rather than to bar spacing as such. The first two sentences of Dr. Hajnal-Kónyi's discussion constitute as adequate a summary of this part of the authors' conclusions as can be stated in brief form: "The tendency of deformed bars to split the surrounding concrete is an inevitable consequence of their improved bond. Full advantage of high bond strength can only be taken if failure by splitting is safely avoided by an appropriate arrangement of reinforcement."

Further information is still needed as to what constitutes this appropriate arrangement of the reinforcement. Fig. 15 suggests that closely spaced stirrups do add much to the bond resistance. The European use of stirrups in all beams, as reported, would certainly decrease the possibility of splitting. But stirrups may be required at spacings much closer than the usual maximum if they are to be fully adequate for this purpose.

In contrast, American practice often uses beams without stirrups and splices without stirrups or ties. Recently some engineers have been using as shear reinforcement U-stirrups without hooks, and the anchorage of such stirrups may itself constitute a splitting hazard. Fortunately such stirrups are generally small bars and not closely spaced, but further investigations as to possible splitting would be desirable.

Recent tests by the authors seem to corroborate Dr. Hajnal-Kónyi's idea that the bar diameter may be of some larger influence than can be covered by expressing bar spacing and cover in terms of bar diameters. The 15 percent lower bond capacity of beam C20 (Fig. 13) with its #11 bar may well be a typical result for large bars.

Mr. Willson's discussion, submitted for the Committee on Reinforced Concrete Research of American Iron and Steel Institute, reviews the circumstances surrounding the development of a beam test by ACI Committee 208 and its use by Clark in the extensive studies that preceded the increase in allowable bond stresses. These were significant developments and AISI is to be congratulated on the contribution it made in this way toward better bars and better understanding. One of the authors had the privilege of visiting a number of these committee meetings and found himself in general agreement with the procedures used and the conclusions reached. But the need for further study of bond stress did not cease to exist after this investigation. Because the investigation used specimens which restricted splitting, its results furnish no guide for members where splitting is a possibility. For such cases, a different design of specimen, one unrestricted against splitting, must be devised and tested. It has been the purpose of this paper to report such tests.

This summer the authors learned of an extensive bond study made by Menzel and Woods and published in 1952.* The opening sentence of their summary states: "Comparison of the first few beam tests with their corresponding pull-out tests indicated that the bond value of a reinforcing bar would not be fully developed unless some sort of auxiliary reinforcement was used in the anchorage zone which would minimize cracking and distortion, maintain beam action and hold the concrete together around the bar." Although this part of the discussion emphasizes diagonal tension cracks, a few pages later splitting is named as a parallel consideration: "The development of longitudinal cracks under the bar or at the sides of the bar often result in splitting of the concrete around the bar and in sudden slip and failure in anchorage."

These statements do not contradict in any way the data presented by Clark, because his data were based on beams where adequate provisions against splitting were incorporated. They do emphasize what the present authors are trying to affirm, namely, that one must exercise discretion in using the results of Clark's work and the present ACI Building Code. They may not be applicable to situations where splitting is less definitely restrained. If they are not applicable, the strong indication is that their use would be on the unsafe side. This is a relatively unexplored field and considerable further research is needed to establish the safe boundaries.

The splitting tendency of deformed bars, either the old types or the new standard bars, is a phenomenon well known for many years. But the nuisance aspects of this splitting action in testing have received more attention than its structural significance.

Some of Mr. Willson's detailed statements require comment. This study was indeed initiated as a study of precast elements, but the paper stated: "The conclusions are in no way restricted to precast units." Thus the authors deem it inappropriate to include a bibliography on precast members or a detailed discussion or reference to bond tests carried out by different methods and under contrasting basic philosophies.

Mr. Willson seems to object to the fact that some two-thirds of the specimens tested were below ACI Code standards for bar spacing and cover, apparently overlooking the fact that this was a study of minimum bar spacing. The small cover used in some cases did result in some shrinkage crack trouble; five of the eight cracked specimens mentioned by Mr. Willson had only 0.375 in. of concrete cover. Shrinkage may have been a factor in more cases, but specimens were carefully watched for any evidence of this kind. Eight cracked specimens, out of a total of 272, represent only a 3 percent loss.

After tabulating 125 eccentric pull-out specimens failing at an ultimate bond stress under 0.10 f_c', etc., Mr. Willson adds the comment: "Since bond stresses at failure normally vary from 0.20 f_c' to 0.30 f_c', these values are quite startling." The authors agree that the low values are quite startling,

*Menzel, Carl A., and Woods, William M., "An Investigation of Bond, Anchorage and Related Factors in Reinforced Concrete Beams," *Research Department Bulletin* No. 42, Portland Cement Assn., Nov. 1952.

at least the first time one meets them. Although some 85 percent of these 125 low values result from tests on substandard bar spacings, the fact that some specimens with standard spacing are included is significant. That corresponding low values occur for beams as well as for eccentric pull-out values, lends credence to the values. By way of contrast, the values of $0.20\ f_c'$ to $0.30\ f_c'$ quoted by Mr. Willson can be documented only from tests where splitting has been restricted by a large concrete mass, by spirals, stirrups, or favorable beam reactions. One purpose of this paper has been to call attention to the major strength differences such restraints introduce.

All specimens were cast with the bars in a horizontal position and, except for two special beams, with the bars in the bottom of the specimen. (Mr. Willson's statement that specimens were cast with bars in a vertical position is in error.)

Mr. Willson also states, without proof, that " . . . test specimens were designed in such a way as to cause unnatural rotations under load applications, thus creating progressive localized excess strains which resulted in abnormal splitting action." The terms "unnatural" and "abnormal" appear to be entirely unwarranted. The authors watched the progress of the eccentric pull-out tests carefully to see whether any unnatural rotations did actually occur under load. They made many observations with dials for lateral movement. It was their conclusion that no significant excess or abnormal strains occurred. For one not close to the tests, the agreement of beam test values (Fig. 13) presents probably the best evidence of the validity of the eccentric pull-out tests. The beam tests were certainly not subject to unnatural rotations or factors of that kind.

If a reader is skeptical of both eccentric pull-out and beam tests, he must still explain the consistent low bond strength found in the four splice beams. Tests on splices have now been extended to some 40 beams, all confirming these preliminary tests.

Finally, the authors must limit Mr. Willson's broad statement that " . . . a great deal of space has been devoted to the presentation of arguments against the further use of the allowable bond stresses specified in the current ACI Building Code." Instead, conclusion 4(a) states that "wherever splitting is prevented, the present ACI Building Code values for bond appear adequate . . . " Only for situations where splitting is possible does the paper raise serious doubts as to the adequacy of this specification. In such cases lower bond stresses or more preventive measures against splitting seem to be indicated.

Referring to cases where lower bond stresses should be specified, the authors originally stated that "further investigation is definitely needed to verify and map such areas with certainty . . . " Many more tests with more variables will be necessary for precise answers to the questions raised by splitting. It is hoped that others will be interested in investigating these variables and defining the phenomenon more closely. In the meantime, some caution is indicated in design. In the case of splices, more conservative designs than the ACI Code now permits are indicated as an immediate measure.

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